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ART. XXXIV.—*On the Surface Geology of Southwest Pennsylvania, and adjoining portions of Maryland and West Virginia;* by JOHN J. STEVENSON, Professor of Geology in the University of New York.

THE following article contains a brief summary of the results obtained by me during three years' labor in connection with the Second Geological Survey of Pennsylvania. The detailed statement will appear elsewhere.

The area in which observations were made covers in all more than 10,000 square miles. It embraces that portion of Pennsylvania lying south from the Ohio and Conemaugh Rivers and west from the Alleghanies; includes a large part of West Virginia and Maryland lying on both sides of the Alleghanies of Virginia; and has the channel-ways of four great rivers, the Monongahela, Cheat, Youghiogheny and Potomac, lying partly within it.

Along all the streams there are fine terraces covered with detritus containing many polished fragments, which have been transported from a greater or less distance; these terraces fall down stream but not so rapidly as do the present stream-beds; and the terraces of tributaries are continuous with those of the main streams.

A second series of benches appears throughout this whole region and seems to be characteristic of a much wider area than that in which observations were made. The benches of this series evidently differ in origin from those of the lower series; their detrital coating contains little clay, no transported fragments and consists almost wholly of sand. They are almost absolutely *horizontal;* they do not merge into the lower series,

but as they often form divides, the lower series ends up in some member of the upper. These horizontal benches begin at 1,100 feet above mean tide; they line the mountain slopes; they curve round the conical hills, and oftentimes they are indicated solely by the leveled crowns of the higher knobs.

As the whole area lies south from the southern line of northern drift, none of that material occurs. In like manner there is no evidence whatever in any portion of the region or at the headwaters of any of the rivers that glaciers were ever present.

The Horizontal Benches.

Benches belonging to this series have been fully recognized over the whole area examined. The altitudes are as follows:—

Above tide.	Above tide.	Above tide.
1. 2,580 feet.	8. 1,570 feet.	15. 1,290 feet.
2. 2,400 "	9. 1,520 "	16. 1,270 "
3. 2,323 "	10. 1,475 "	17. 1,240 "
4. 2,288 "	11. 1,445 "	18. 1,190 "
5. 2,063 "	12. 1,420 "	19. 1,130 "
6. 1,820 "	13. 1,380 "	20. 1,100 "
7. 1,690 "	14. 1,350 "	

Of these benches, those below No. 11 were recognized at many localities within an area of more than 5,000 square miles; the higher ones were seen at widely separated localities, but their level is reached only on the sides of the mountains, or in elevated valleys like the Ligonier or the Salisbury Basin. The extreme variation in level is barely twenty feet in any case and in most of the benches the altitude is accurately the same at all places. No. 17 shows a variation of eighteen feet, and I am much inclined to believe that I have confounded two benches which are close together. These benches are parallel.

The arrangement of the horizontal benches is finely shown on the National road between Chestnut Ridge and a high hill fifteen miles west from the Monongahela River.

Standing on the highest point crossed by that road between Chestnut Ridge and the Monongahela River, one finds himself standing on an island of No. 14, or 1,350 feet above sea-level. Below him, an almost continuous plain of No. 15 stretches for a long distance north and south and is broken only by gaps which the larger streams make through this small ridge on their way to the Monongahela River. He sees also that this plain is the divide between two valleys, one at the east between this and Chestnut Ridge, and the other at the west, in which the Monongahela flows. The latter is uninterrupted as far as the eye can reach, but the former is crossed by strips of the main plain of Brush Ridge, as well as by lower benches which break its continuity and convert it into a succession of basins.

From the summit-plain of this Brush Ridge, the surface falls off in regular steps.

If, now, the observer turn his attention to the region lying directly west from the Monongahela River, he will see that No. 15 is a broad continuous plain beyond that river, but that still farther back toward the west, the fourteenth bench, on an island of which he is standing, forms a similar plain, while still farther back, No. 13, with an altitude of 1,380 feet above tide, stretches northward and southward and is broken only by the narrow valleys in which the larger streams flow.

Should the observer's position be changed to Hillsborough, fifteen miles west from the Monongahela River, where the elevation is about 1,500 feet above tide, he will see that No. 13 is of great extent north and south, while back of it the country rises to a still higher level, again and again, until it reaches No. 11 at 1,445 feet above tide.

From the river westward to Hillsborough, or rather to a ridge passing nearly north and south at three miles east from that village, the surface rises in a succession of steps which are beautifully marked. From the hill-top at Hillsborough, the descent to the river is very handsomely shown.

That these benches are simply the result of remodeling valleys formed long before the agent making the benches began to work, is shown by the distribution of the benches themselves: for these benches line the sides of long narrow valleys reaching far inland from the rivers, and breaking through ridges bearing higher members of the series. The erosion producing these valleys began, I believe, even before the anticlinal axes had been elevated sufficiently to affect the topography. The main streams of the present drainage system break through all the bold axes west from the Alleghanies of Virginia.

The recent origin of the benches is amply clear from their condition of preservation. If they had been of ancient origin their detrital coat would have been removed, for we cannot doubt that rain fell on these as well as along the lines of the water-courses; in like manner the crowns of the hills would have been rounded, especially where the rock is a soft shale inclined at a considerable angle, as is the case in southern Fayette county.

The whole structure of these benches shows them to be simply old beach lines marking successive stages of emergence from a flood of waters. They are horizontal, parallel, and are covered with detritus derived from the immediate vicinity. They extend over an enormous area both east and west from the Alleghanies of Pennsylvania, which, within that State at least, form the water shed between the Atlantic Ocean and the Gulf of Mexico.

As they are so wide-spread, it is impossible to account for

them by the draining away of a great lake or by the action of a great flood sweeping over the whole region. They can be no other than sea-beaches, marking stages in the withdrawal of the ocean. This supposition involves a submergence of the land to a depth of fully 2,600 feet, if we regard the higher benches as due to the same cause with the lower ones, and the submergence would have to be somewhat greater to account for the even crests of the Alleghanies and other ridges of the Appalachian region west from the Blue Ridge.

The River Terraces.

The persistent terraces are five in number and their relations are shown at the junction of Cheat and Monongahela Rivers. Three or four miles north from the West Virginia line, they are as follows:—

Above river.	Above river.
1. 280 feet.	4. 80 feet.
2. 210 "	5. 20 "
3. 180 "	

The absolute elevation of the highest terrace at this locality is 1,050 feet above tide.

These fall down stream and are covered by detritus, consisting of irregularly bedded sand, clay or gravel, in which are transported fragments which have been rounded by the action of running water. When followed up any of the smaller, or for long distances on the larger, streams, these terraces are seen to differ in degree of slope, so that each lower one is merged into the next higher until that, which at the mouth of the stream is the river "bottom," becomes the only terrace and at last is lost in the lower horizontal benches where the stream takes its rise.

The terraces occur at the same elevation on both sides of streams, being divided by the channel-way just as the present "bottom" is divided. In some instances a terrace is wanting on one side; but there it is clear enough that that corrasion was confined to one side, for the terrace is unusually wide on the other. The same condition is often seen in the flood plain of the river now.

As stated in my report for 1875, these terraces are simply shelves in the rock on which rests a thin coat of detritus. Mr. G. K. Gilbert, in his memoir on the geology of the Henry Mountains (not yet published), describes similar terraces as occurring there, though it does not appear that they are found at the same height on both sides of the streams.

The terraces on the Ohio, below Pittsburg, consist largely of northern drift brought down by the Allegheny and Beaver Rivers, so that they certainly date from a time later than that

at which the drift was spread over northern Pennsylvania. Along the Monongahela and other rivers south from the Ohio, no such material occurs, but the deposits afford sufficient evidence of another kind to enable us to fix their origin within comparatively recent times. At New Geneva on the Monongahela near the West Virginia line, the highest terrace has a thick coating, in which a layer known as the "swamp clay" holds much half rotted wood, such as is frequently seen in peat bogs. In the same neighborhood the third terrace shows many *Unio* shells in an advanced stage of decay. A similar condition exists on the same terrace at Morgantown, farther up the river in West Virginia. At Belvernon, on the same river, near the northern line of Fayette county, this terrace yields many fragments of wood. In this way it can be shown that the deposits on the first, third, fourth and fifth benches are of recent origin.

Since these deposits are of recent origin, there would seem to be good reason for supposing that the valleys through which their streams flow are also of recent origin, at least so much of them as lies below the level of the highest terrace. But it has been suggested that these terraces are only the result of reworking the sides of the valleys, which had been eroded previously.

The structure of the valleys below the highest terrace is very different from that above that terrace; for in the upper portion the sides are gently sloping, whereas below it they become steep at once. Above the line of that terrace, the smaller valleys are broad swales, while below it the streams flow in gorges. The abruptness of this change from gentle to steep walls shows that the lower portion of the gorges was eroded after the upper portion had acquired approximately its present form. There is not the slightest atom of evidence to lead any one to suppose that the valley of the Monongahela was ever filled with gravel, and the stream now flows on a rocky bed from its source to within a very few miles of its mouth.

These river terraces are relics of river beds, which at one time stretched across the valleys, just as the river "bottoms" now do; and the valleys below the line of the highest terrace have been eroded since the drainage system was reëstablished by withdrawal of the ocean below the lines of the former stream beds.

Conclusions.

The general conclusions to which I have come are:—1. That the erosion, to which is due the general configuration of the surface above the highest river terrace, began even before the elevation of the anticlinal axes and continued until the whole region was submerged in post-glacial time. 2. That the horizontal benches are due to re-working of preëxisting valleys,

and that they mark stages of rest during emergence of the continent from the ocean which covered it. 3. That the river terraces and the valleys which they line, were formed after the drainage system had been reëstablished by withdrawal of the water to a level below that at which the streams had previously flowed.

It will be seen at once that this last conclusion leads to one of wider application.

So long a time elapsed between the beginning of the drainage and the coming of the great flood, that deepening of the water-ways kept pace only with the general wasting of the adjacent country, for we find comparatively gentle slopes down to the line of the highest terrace. But after the drainage had been reëstablished, the rate of flow must have been more rapid than previously, so as to increase the corrasive power of the streams to far beyond what it had been, for in the newer portion of the valleys the sides are abrupt. There must then have been a change of altitude in respect to tide-level, to lead to this increased rate of flow and the consequent increased speed with which the channel ways were deepened.

It would appear then that, after the submergence following the Glacial period, the continent rose to a greater height than it had before the submergence, or that the ocean was drawn off to a lower level than before; the result in either case being the same—to depress the mouths of the great rivers, to increase the fall of the streams and therefore to cause the rapid deepening of the water-ways.

Art. XXXV.—*On the Driftless Interior of North America;* by James D. Dana.

1. *Driftless area of Central and West-Central North America.*

In the number of this Journal for April, 1875,* I have accounted for the absence of the northern drift from the interior of North America—over the great region between Western Iowa and the Sierra Nevada in California and the country north to an undetermined distance—on the ground of the dryness of the climate in connection with the heat of the summer; and I referred for the facts on the former of these points to Mr. Charles A. Schott's very valuable memoir on the "Precipitation in rain and snow in the United States," published in 1872 by the Smithsonian Institution. As the subject is one of great interest I here reproduce portions of two of Mr. Schott's charts (see beyond); one (No. 1) giving the lines of equal precipitation for

* Vol. ix, p. 312. Further, vol. x, p. 385, and vol. xiii, p. 80. The connection between the distribution of the ice and the amount of precipitation is appealed to also in ibid., v, 206, 1873, and illustrated from Mr. Schott's chart.

the winter months, December, January and February, and the other (No. 2), the same for the year. The number of inches along the course of each line is marked on the line.*

It will be observed on the chart of the *winter* precipitation (No. 1) that the area of *two* inches or less for the precipitation of the winter months spreads into Western Iowa and part of Minnesota. On Mr. Schott's complete chart, it is shown to cover the whole area (some mountain ridges not being considered) from Central Kansas nearly to the Sierra Nevada; from Iowa and Minnesota to Salt Lake City; and from Minnesota northward and westward. The line of *four* inches, as the copied portion of the chart shows, passes along the eastern border of Kansas, and through Central Iowa and Wisconsin, and thence northward. It runs north of Salt Lake City near the line of *two* inches for 150 miles, and thence—while the latter diverges eastward—continues northward along the summit of the mountains.

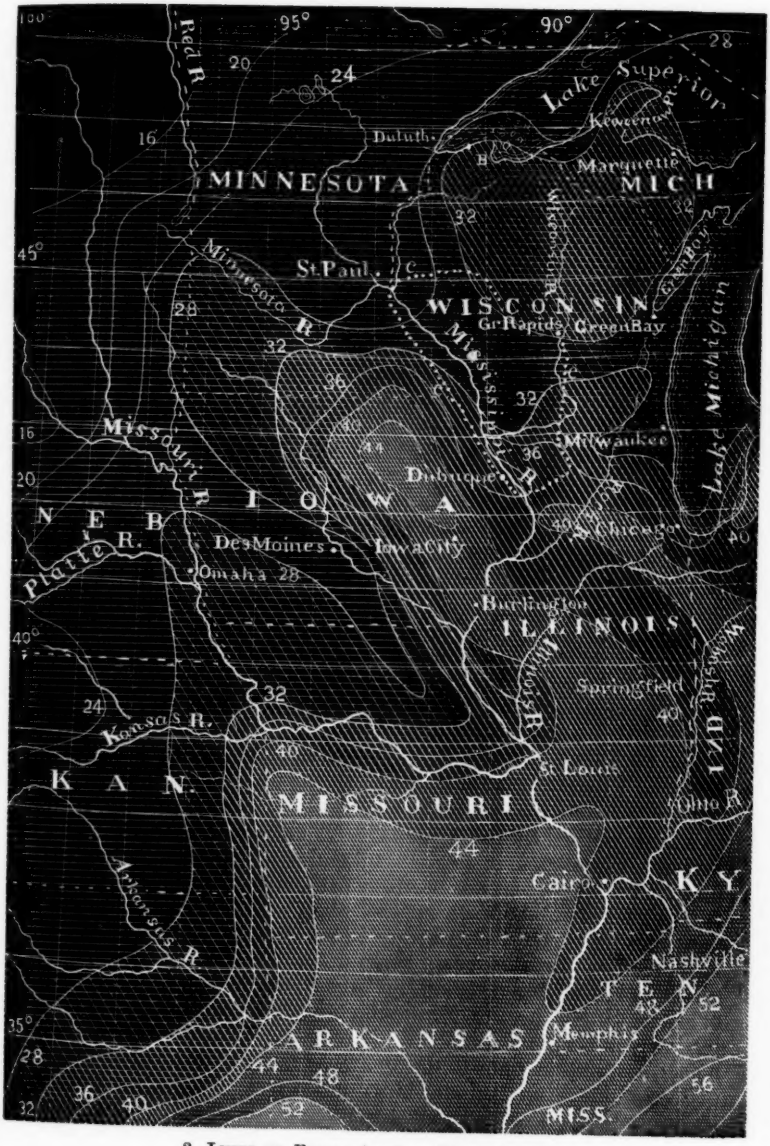
In contrast with this, the winter precipitation over New England is 8 to 12 inches; over New York, 6 to 10 inches; over Ohio and Indiana, 8 to 10 inches; over the Southern States, from Virginia to Georgia and Louisiana, 10 to 20 inches.

On the chart No. 2 is given the amount of *annual precipitation* for this same region. While this amount is 40 to 45 inches in New England; 40 to 50 inches from Pennsylvania southwestward; and 40 to 45 over Ohio and Indiana, the line of 20 inches (half the average for New England, Pennsylvania, Ohio, and less than half for the more Southern States) crosses Western Minnesota and passes just west of Iowa; and the line of 16 inches enters Minnesota. Mr. Schott's chart shows that the area over which the precipitation is only 12 inches or less up to 16 inches embraces (exclusive of parts of the Rocky Mountain summit not considered) the wide area from the meridian of 96° in Minnesota and from that of 98° in Southern Dakota, to the meridian of 121° in Oregon, or 23 to 25 degrees in latitude; and from the meridian of 102° in Nebraska and Kansas to Virginia City in Nevada, near the meridian of 117°. North of the United States the lines are not given; but it is well known that the breadth of the area does not diminish in that direction.

At the same time it is to be considered that the *isothermal* lines, and especially those of the summer, bend far northward over the dry region. The climate consequently would have necessarily occasioned over this central and western area of the continent, only a small amount of precipitation in the Glacial era; and all the observed glacial facts prove positively that it

* In the western portions, the line 2 of No. 1, and 16, 20 and 24 of No. 2, are, according to Mr. Schott, only approximations.

1. Lines of Equal Winter Precipitation.



2. Lines of Equal Annual Precipitation.

was too small for the production of a southward moving glacier. The southwestward direction of the scratches and of the bowlder-movement over the area from Wisconsin to Lake Winnipeg not only sustains this, but shows also that the ice had its greatest height over the region of greatest precipitation somewhere between the line from Wisconsin to Lake Winnipeg and beyond, and that of the Atlantic coast. More facts are needed before the northern limit of the glacierless and driftless area can be laid down.

2. *Driftless area in Wisconsin.*

The charts accompanying this paper have been introduced here partly to exhibit the bearing of the climatal facts on the question as to the origin of the "driftless area" in Wisconsin, a description of which is given, from Professor Irving's Report, on page 61 and beyond of this volume. The outline of this driftless area is indicated on the two maps (Nos. 1 and 2) by a dotted line, lettered *c*, *c*, *c*, *c*. It is a striking fact that on the *winter* chart (No. 1) the driftless area is almost wholly included within the area which has only 2 to 4 inches for the amount of winter precipitation; that this area of 2-to-4 inches fall extends in that direction like a deep bay between areas of greater precipitation. To the northeastward of it the amount of precipitation increases from 4 to 10 inches, the last being that on Keweenaw Point, and southward and southwestward over Illinois and part of Iowa to 6 and 8 inches. All of the Wisconsin driftless area, excepting its southern portion, is within the 2-to-4 area. Again, on the chart of *annual* precipitation (No. 2), this driftless area (excepting its south end) is the driest part of Wisconsin, the amount of precipitation over it being below 32 inches, while it is 32 inches or over to the northeast, as far as Keweenaw Point, and to the south and southwest.

It seems therefore probable that the driftless area was located to a large degree by the climate. At the same time Professor Irving's suggestion is needed to make the explanation complete. For this area was not a continuation southeastward of the great central driftless area of the continent, as it is hygrometrically, but, on the contrary it was wholly surrounded by moving ice; for the ice is proved by bowlders to have extended on the *west side* of the area from the southwest end of Lake Superior over Minnesota and Iowa, and even beyond into Missouri where large bowlders occur. The locality of the mass of native copper found in Lucas County, Southern Iowa, is distant four hundred and sixty miles from Keweenaw Point, its probable source. From Iowa the ice stretched eastward across Illinois to the Lake Michigan region.

This southwestward prolongation of the glacier from the western half of Lake Superior over a region as dry as that of

the Wisconsin driftless area appears to be a consequence, as Professor Irving urges, of the great depth of the Lake Superior trough—over a thousand feet below the present surface of the water—and its lying in a southwest-by-west (or about S. 55° W.) direction, which was nearly that of the glacier motion in that part of North America. For this would have determined the movement of a great ice-mass southwestward, over the northern half of Wisconsin, and over Minnesota, and thence across Iowa, where there was again abundant precipitation. The annual precipitation is 32 to 40 inches over Eastern Iowa, Illinois, and the region about the southern part of Lake Michigan.

With such a driftless area within the glacier limits—an area without depth of ice enough for movement—the pitch of the upper surface of the ice around it would have been quite various. The height must have been great enough in the eastern part of the Lake Superior region and north of Keweenaw Point to have determined movement through Minnesota and Iowa to Missouri, a distance of five hundred miles, this being shown by the bowlders of copper. Whatever the pitch along that course, it was twice as great toward the Wisconsin driftless area, since the northern border of the area is hardly half as far. Down Lake Michigan the pitch continued into Illinois and Indiana; but the Kettle Range west of Lake Michigan, running along the east front of the driftless area, marks out, as Professors Chamberlain and Irving show, its moraine termination in that direction.

The eastern parallel branch of the Kettle Range lying between the Green Bay Valley and Lake Michigan, which, according to these geologists, is also a moraine ridge, is evidence, as they observe, that at the time when it was formed, the glacier of Green Bay Valley was distinct from that of Lake Michigan. It seems probable that when the Glacial era was at its height, the two were merged in one glacier; but that later, as the ice diminished, the former became independent, and that then the eastern Kettle Range was made.

3. *The earth's axis had the same position in the Glacial era as now*, if the driftless character of the Wisconsin area depended on the climatal conditions explained. The concordance between the limits of the drier areas of the Glacial era and those of the present time, and especially the fact in this respect with regard to the isolated area in Wisconsin sustains this proposition. The probability that such was the truth was long since made apparent by the observation that the southern termination of the glacier in North America and Europe was very nearly along what is now the course of the same identical isothermal. The position and extent of the Wisconsin driftless area afford more precise and positive evidence.

ART. XXXVI.—*The Ancient Outlet of Great Salt Lake;* a letter to the Editors, by G. K. GILBERT.

GREAT Salt Lake has no outlet, and its fluctuating level is determined by the balance between inflowing streams and solar evaporation. On the surrounding mountains there are water-lines rising in steps to a thousand feet above its surface, and showing that in ancient times a great body of water occupied its basin. This ancient body, known as Lake Bonneville, was 345 miles long from north to south and 135 miles broad, and its vestiges are on so grand a scale that they have attracted the attention not only of geologists but of every observant traveler. It naturally occurred to many persons to enquire whether the lake waters did not in their flood stage find an outlet, and several theories have been advanced in regard to it; but previous to 1876 the outlet was not discovered, or if discovered its position was not announced. In the summer of that year I left Ogden for the purpose of seeking the outlet at the north, and in a few days had the great pleasure of finding it in Idaho, at the north end of Cache Valley, the locality being known as Red Rock Pass. The circumstances were such as to leave no doubt in my mind that I had determined the actual point of outflow, and on my return to the East I made the announcement without reservation in a communication to the Philosophical Society of Washington. The announcement was also made for me in the same unequivocal manner by Professor Joseph Henry, in the Smithsonian Report for 1876 (p. 61), and by Professor J. W. Powell, in Baird's Annual of Scientific Discovery for 1876 (p. 260), and there seemed no occasion for further publication until the matter should receive its full discussion in the Reports of the Survey of which Professor Powell has charge. But, in the American Journal of Science for January, 1878 (p. 65), there appears a statement (apparently on the authority of Dr. F. V. Hayden, but without signature) that "it is believed that the explorations of the Survey, under the direction of Dr. Hayden, the past season, have determined the probable ancient outlet of the great lake that once filled Salt Lake Basin;" and there is so much doubt implied by the use of the phrases "it is believed" and "probable outlet" that it seems proper for me to defend my positive assertions by setting forth the facts which appear to me to place the existence and position of the ancient outlet beyond question.

If Lake Erie were to dry away, and a geologist of the future should examine its basin, he would easily trace the former shoreline around it. At two points he would find this line interrupted. At Detroit and at Buffalo he would meet with narrow, trough-

like passes, depressed somewhat below the level of the shore line, and leading to other basins. Following the Detroit Pass he would be led to the Huron basin and would find there a shore line so nearly on a level with the Erie that he could not readily determine which was the higher. Following the Buffalo Pass he would find a continuous descent for many miles to the Ontario basin, and in that basin he would find no water-line at the level of the Erie shore. In each case he would learn from the form of the passage that it had been the channel of a river, and in the latter case he would learn from the direction and continuity of descent, and from the absence of corresponding shore lines, that it had been the channel of an *outflowing* river.

So in regard to Lake Bonneville. To discover its outlet it was necessary to find a point where the Bonneville shore line was interrupted by a pass of which the floor was lower than the shore line, and which led to a valley not marked by a continuation of the shore line. These conditions are satisfied at Red Rock Pass, and, in addition, there is a continuous descent from the pass to the Pacific ocean. All about Cache Valley the Bonneville shore line has been traced, and it is well marked within a half mile of the pass. The floor of the Pass at the divide is 340 feet below the level of the shore line, and its form is that of a river channel. The gentle alluvial slopes from the mountains at the east and west, which appear once to have united at the pass, are divided for several miles by a steep-sided, flat-bottomed, trench-like passage, a thousand feet broad, and descending northward from the divide. At the divide Marsh Creek enters the old channel from the east, and turning northward runs through Marsh Valley to the Portneuf River, a tributary of the Columbia. In Marsh Valley the eye seeks in vain for the familiar shore lines of the Salt Lake Basin, and the conclusion is irresistible that here the ancient lake outflowed.

At the divide a portion of each wall of the ancient channel is composed of solid limestone, and its floor is interrupted by knolls of the same material. It is evident, too, that the channel has lost something in depth, for Marsh Creek and some smaller streams at the south have thrown so much debris into it as to divide it into several little basins occupied by ponds and marshes. It is not improbable that twenty or thirty feet have thus been built upon the floor and that the original bed of the channel where it crosses the limestone is 360 or 370 feet lower than the highest Bonneville beach. Still we must not suppose that the floor of the outlet was ever 370 feet below a coexistent level of the lake, but rather that during the existence of the outlet its channel was slowly excavated to that extent, while the lake was to the same extent drained. This view is sustained in a very striking manner by the phenomena of the shore lines.

From the highest shore line, known as the "Bonneville Beach," down to the level of the modern lake, there is a continuous series of wave-wrought terraces recording the slow recession of the water. As many as twenty-five have been counted on a single slope. Some are strongly marked and others faintly, and some that are conspicuous at one point fail to appear at other points; but there is one that under all circumstances asserts its supremacy and clearly marks the longest lingering of the water. It has been called the "Provo Beach," and it runs about 365 feet below the Bonneville Beach. When the discharge of the lake began, its level was that recorded by the Bonneville Beach. The outflowing stream crossed the unconsolidated gravels that overlay the limestone at Red Rock Pass, and cut them away rapidly. The lake surface was lowered with comparative rapidity until the limestone was exposed, but from that time the progress was exceedingly slow. For a long period the water was held at nearly the same level, and the Provo Beach was produced. Then came the drying of the climate, and the outflow ceased; and slowly, with many lingerings, the lake has shrunk to its present size.

In Dr. Hayden's Preliminary Report of the field work of his Survey for the season of 1877, noticed on page 56 of the current volume of this Journal, there is no mention of the observations at Red Rock Pass, but the omission appears to have been accidental, for on page 7 he says: "At the divide between the Malade and Marsh Creek is another of the old outlets of the ancient Salt Lake when its waters were at the highest level." This passage occurs in a summary of Dr. A. C. Peale's geological observations, but it is to be hoped that the idea will not be advocated in that gentleman's report. The divide referred to is near Malade City, and separates Malade Valley from Marsh Valley. The Bonneville Beach is well marked all about Malade Valley, and nowhere more strongly than in the vicinity of Malade City. It runs between that place and the divide at an altitude of about 400 feet (by barometer) above the city, while the divide, as determined by Dr. Hayden's assistants, has an altitude above the city of 950 feet. After making every allowance for the errors incident to barometric determinations of altitude, it must be conceded that the divide is several hundred feet higher than the water line. It appeared so evident from a distant view that the lake did not overflow this ridge, that I did not ascend to the summit, although I had undertaken last summer to examine every divide between the Columbia and Salt Lake Basins that might possibly have afforded passage to the water. I am aware that Prof. F. H. Bradley, who visited the locality in 1872, expressed the half formed opinion that it had been a point of outflow, but he de-

scribed no channel of outflow; and it is evident, moreover, that he gave little thought to the subject, for he made the somewhat astonishing suggestion that four outflowing streams might have coëxisted—one at the Soda Spring Pass, one at Red Rock, one near Malade City, and one at the head of the Malade River. If he had seen the channel at Red Rock, I do not doubt that he would have recognized it as the real avenue of discharge.

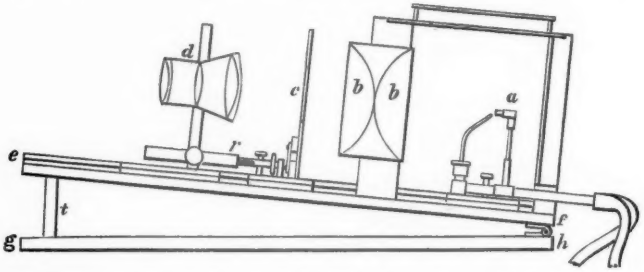
It is proper to add in this connection, that I have been able to demonstrate that certain small orographic movements have transpired in the Bonneville Basin, not only since its desiccation but during its flooding, and that it is perfectly conceivable that such movements shifted the outflow from point to point. To ascertain whether they actually did so, I have traced out during the past summer all of the shore line that had not previously been explored, and in so doing I have satisfied myself that the only outlet of Lake Bonneville was at Red Rock Pass.

ART. XXXVII.—*On the Projection of Microscope Photographs;* by JOHN CHRISTOPHER DRAPER, M.D., LL.D., Professor of Natural History in the College of the City of New York.

IN the lanterns that are constructed for the projection of photographic or other images on a screen, the support or stage on which the photographic slide is placed is close to, and at an invariable distance from, the condensing lens. So long as the objects to be projected are nearly equal in size to the diameter of the condenser, this is the only adjustment that can be made to illuminate the whole surface of the object; but, when the diameter of the field occupied by the object is only one-half, or one-quarter of the diameter of the condensing lens, the brilliancy of the result obtained upon the screen may be greatly increased by removing the supporting stage or object carrier to a greater distance from the condenser, so that a convergent beam of light may fall on the object to be projected. To accomplish this I have constructed the following form of lantern:

In the figure, *a* is a zirconia light, mounted on an adjustable base (see American Journal of Science and Arts, Sept., 1877, page 208), which may be used with a condensing lens of very short focus, since the zirconia is not burrowed into cavities where the oxyhydrogen flame impinges, as happens with lime cylinders, and causes the flame to be reflected upon the condensing lens and thereby destroys it. In the jet employed, the gases are mixed just before they are ignited. *b*, *b*, is a short focus condensing lens. *c*, the stage or support carrying the photographic or other design to be projected. *d*, the projection

lens formed of three sets of lenses and giving a perfectly flat rectilinear field. *a*, *c*, *d*, are mounted on a base board *e*, *f*, to the end of which the lantern box *a*, *b*, is attached, and which is freely open above and below to permit perfect ventilation. The base carries lateral grooves in which *a*, *c*, *d* slide, allowing them to be placed at varying distances from *b*, and fixed by suitable binding screws. *c* and *d* are also connected together by a rod *r*, carrying an adjustment screw at *r*, by which the change of distance between *d* and *c*, required in giving the correct focus, may be obtained. The base *e*, *f*, is attached to a second or



under base *g*, *h*, by a hinge at *h*, which allows the end *e*, of the movable base *e*, *f*, to be raised to any required angle; at which it may be maintained by the block at *i*. So convenient and compact is this lantern that it may easily be stowed away in a small trunk.

When a series of objects of very different sizes is to be projected, as is the case with microscopic photographs taken under the same adjustments, it is, as we have said, a great gain in the projection of small objects if the circle of light used for illumination is reduced; and, at the same time, increased in brilliancy. This is accomplished in the above arrangement as rapidly as can be desired by removing *d*, *c*, together, along the slide of the base *e*, *f*, to a sufficient distance from the face of the condenser *b* to allow the convergent rays from the latter just to cover a circle which will include the object to be projected. The greater intensity of the illumination thus obtained renders the definition of fine markings or other peculiarities on small objects as clearly visible at considerable distances as are the coarser markings on large objects under a weaker light.

In closing this brief communication I desire to add that I have made photographs of *Frustulia saxonica* under a power of 7,000 diameters. The photographs in question were made in the City College building by a one-twentieth inch immersion Beck lens. The light was from the sun, reflected by a heliostat, through ammonio-sulphate of copper solution, and con-

densed on the object at an angle of 30° to 40°. The photograph in question was direct, by which I mean that there was no intermediate or secondary enlargement of a first photograph. With this photograph and the lantern described, I have shown *Frustulia saxonica* magnified more than half a million diameters; a result which must be seen to be appreciated.

ART. XXXVIII.—*On the Discovery of Lower Silurian Fossils in Limestone associated with Hydromica slates, and on other points in the Geology of Lehigh and Northampton Counties, Eastern Pennsylvania;* by FREDERICK PRIME, Jr., Professor of Metallurgy at Lafayette College, Easton, Pennsylvania.

[Professor Prime's discoveries were made in connection with his work, during the past season, as Assistant Geologist in the Geological Survey of Pennsylvania. The region in which the discoveries were made lies near the eastern boundary of the State, southeast of the Blue Ridge. The rocks are related in position, in lithological character, and also in their direct association with large beds of limonite, to those of Vermont in which the discoveries of fossils by Mr. Wing were made, and to the continuation of these Vermont rocks southward through Berkshire, Massachusetts, near and along the Taconic range. The discoveries were first made known to the writer by Professor Lesley, the able director of the Pennsylvania Survey, and by Professor Prime, in November last; and both recognized the intimate geological as well as geographical relation between these East-Pennsylvania and West-New England rocks. Professor Lesley stated in his letter that the opinions which he had derived, from the observations of others, more than thirty years since with regard to New England geology, he now (since the discovery of fossils in limestones among the metamorphic rocks of Vermont, of Bernardston and Littleton in the Connecticut Valley, and of Eastern Pennsylvania) regards as greatly strengthened in probability—namely: That Paleozoic rocks make up the Green Mountains, and also the White Mountains, and that the latter include beds of Devonian age. Professor Prime's article, which is here published, is cited from the Proceedings of the Philosophical Society for December 21, where it appears under the title "*On the Paleozoic Rocks of Lehigh and Northampton Counties, Pennsylvania.*—J. D. D.]

THE Paleozoic rocks of Lehigh and Northampton counties are: The Potsdam Sandstone (No. I); Magnesian or Auroral Limestone (No. II); Trenton Limestone (No. II); Utica Shale (No. III); Hudson River or Matinal Slate (No. III).

The Potsdam sandstone is first found in the outlying peninsula of the South Mountains, known as Lock Ridge, where it

occurs on the northwest flank of the hill and undoubtedly has a northwest dip. It next occurs in two small patches on the northern flank of the main range of the South Mountain near Macungie (formerly Millerstown). A small patch of it is also found associated with the gneiss, where the latter crops out through the limestone in the gorge of the Little Lehigh Creek at Jerusalem Church, two miles northwest of Emaus. But it is first seen to any great extent along the north flank of the main range just south of Emaus, where its occurrence is constant, but of varying thickness, and continues for a distance of four and a half miles, after which it can no longer be traced.

It occurs again at the ridge of the South Mountain, close to Allentown, which forms the southern barrier of the Lehigh River, between Allentown and Bethlehem, where the sandstone is about twenty-five feet thick and extends with a few intervals (where it has been cut out by the river) the entire distance between these two places. It also extends across the Lehigh and forms the capping rock of a portion of the gneiss just east of Allentown and north of the Lehigh. The contact between the gneiss and sandstone is distinctly seen about two miles east of Allentown on the Lehigh Valley Railroad track.

The very lowest beds of the Potsdam sandstone are actual pudding-stones, containing quartz pebbles the size of a man's fist and larger, and fragments of red, unaltered orthoclase. The upper beds are composed of a hard, compact quartzite containing greater or less quantities of feldspar nodules, which weather out and impart to the rock a pock-marked appearance. When first quarried the color of this quartzite is blue to bluish-gray, which on exposure soon changes to a dark reddish-brown, due to the oxidation of the ferrous oxide it contains. The change from a pudding-stone to a compact quartzite in the sandstone shows that there has been a gradual sinking of the earth's crust and an increase in the depth of the sea, thus preparing the way for the subsequent deposition of the limestone.

The Potsdam sandstone often, as elsewhere, contains *Scolithus.*

Next above the Potsdam sandstone occur *hydromica slates*, which Rogers has called the Upper Primal Slates, but *which really form a portion of the No. II limestone*, and gradually pass into this. They lie along the north flank of the South Mountain and overlie the Potsdam conformably wherever this is visible, being far more persistent in their occurrence, continuing with few intervals the entire distance from the western boundary of Lehigh county to the Delaware River. They are of great economic importance as carrying the lowest range of brown hematite iron ores, to be mentioned later.

These slates are composed in great part of the mineral damourite and occur of a pink, gray, white and yellow color.

When exposed to the weather they very rapidly decompose to soft unctuous plastic clays in a few days, and some of these will in time probably become valuable in the manufacture of coarse kinds of pottery. Generally they contain more or less of the carbonates of lime and magnesia, and silica, mixed with the damourite.* Hydromica slate also occurs the greater portion of the distance from the western boundary of Lehigh county to the Delaware River, at the junction of the No. II limestones with the No. III slates, here also carrying brown hematite ores in extensive deposits.

It also occurs intercalated in the limestone, forming layers from the thickness of a sheet of paper to several feet, and these layers are innumerable. Their existence has been seen both in rock outcrops as well as in wells which have been sunk.

The clay to which the hydromica slate decomposes is generally of a white color, although sometimes brown from the presence of hydrated ferric oxide. Analyses would seem to show that the clay contains rather less potash than the undecomposed rock.

Overlying the hydromica slates, and conformable with these and the Potsdam sandstone, is the No. II or Magnesian limestone (Auroral of Rogers), which extends as a great mass varying from six to seven and a quarter miles in width. At four points gneiss crops out through the limestone. These are at Chestnut Hill north of Easton, at a hill two miles north of Bethlehem, the gneiss ridge north of the Lehigh, between Allentown and Bethlehem, and at Jerusalem Church, two miles north of Emaus. Otherwise its continuity is unbroken.

In its lower beds the limestone contains large quantities of chert, forming nodular masses of very various sizes and usually having their longest axes conformable to the bedding of the enclosing rock. This chert occurs in the manner described by Safford† as characteristic of the Knox dolomite of Tennessee. It disappears, however, in the upper strata.

The limestone varies from a blackish-blue to dove color, being for the most part compact to semi-crystalline, while there are occasionally shaly beds. In composition it varies much, often approaching a true dolomite, again a pure limestone. But from the isolated analyses made it would seem as if the percentage of magnesia was less in the upper beds than the lower ones. The limestone is always siliceous, often very much so, and hence much care is now being taken by many of the iron-masters in selecting beds of it, which are low in silica, so as to be suitable for smelting the iron ores of the Great Valley and New Jersey, which are high in silica. It often contains minute

* Report of Progress for 1874 of Lehigh Dist. Geol. Survey of Penn., p. 12.

† See Geology of Tennessee, by Safford, pp. 215, 218.

grains of pyrite disseminated through it, which weather out on exposure, leaving minute cavities behind. Numerous analyses have shown the presence of ferrous carbonate varying in amount from 0·538 to 1·305 per cent.

A peculiarity of the limestone is that it is often brecciated, the fragments being composed *exclusively of limestone*, cemented together by calcite or dolomite. The brecciated appearance is rarely visible on fresh fracture, being usually brought to view by weathering. When seen in place it will usually be found that one or more brecciated beds occur between two others which do not exhibit this peculiarity. As the beds of the No. II limestone have been much disturbed by the force which elevated the South Mountain range, the probable explanation of this brecciation is that a very hard, unyielding bed occurs between two more pliable ones; that these, when subjected to the lateral thrust of the uprising mass of the South Mountains, have conformed themselves to the folds of the strata, while the harder one, being unable to do this, has been fractured and *recemented in sitû* by the percolation of calcareous waters.

Some observers have supposed that the No. II formation is actually composed of two limestones, the lower one belonging to the Huronian, the upper to the Calciferous; and patches of the latter are supposed to overlie the former. The upper limestone (according to these observers) having been formed from the lower, the brecciated limestones are adduced as evidences of upheaval and shore action.

The explanation I have offered of the formation of the brecciated limestone is both more in accordance with the facts observed and with the generally accepted view of the deep-sea formation of limestone than the hypothesis above stated; for the brecciated limestones are as common near the base of the series as at the top.

Besides, the genus *Monocraterion* found in the Lehigh county limestone belongs to the same family as *Scolithus*, and is therefore no greater proof of age than the latter; and it occurs in but one locality close to the top of No. II, being not more than fifty to one hundred feet from the overlying Calciferous and Trenton.

The fossils thus far found in the No. II limestone do not number a dozen specimens, and have been found in but four localities. At Helfrich's Spring, about two and a half miles north of Allentown, the Jordan makes a great bend around a limestone hill, and, by an underground passage of a portion of its waters, has excavated a cave a short distance into it. At the west end of the hill, near the small opening where that portion of the creek forming the spring disappears, there occurs a new species of *Monocraterion*, as yet undescribed. Of this half a

dozen casts have been found; but all efforts to discover the fossil itself have been hitherto unsuccessful. This discovery is the more interesting as the genus *Monocraterion* has hitherto only been known to occur in Sweden.

About half a mile northeast of this five or six specimens of a *Lingula* were found in John Schadt's quarry, but it is impossible to determine its species. About half a mile west of Helfrich's Spring a single specimen of an *Orthoceratite* was found close to the Jordan, just north of Scherer's Tavern, but so imperfect that its species is undeterminable. Finally a specimen of *Euomphalus* was found on Nero Peters' farm, two miles east of Ballietsville.

Not a single fossil has thus far been found in the No. II limestone of Northampton county.

The No. II limestone, like the Magnesian limestone of the Mississippi Valley, is exceedingly soluble. Streams constantly disappear in the ground, forsaking their original beds except when the volume of water is too great to be carried off by the subterranean channels, only to reappear again as springs at greater or less distances. The effects due to this solution of the limestone are very great. Not only are small sink-holes very common, but beds are found often much contorted locally in a manner which can only be explained by supposing them to have dropped down by their own weight into caverns excavated by the water beneath them. Possibly also the contortion of the hydromica beds as developed in the brown hematite mines at the junction of the limestone with the No. III slates is due to the same action, rendered more prominent by the passage of streams from the slate to the limestone, where the solving action could begin. The different beds too are soluble in very different degrees; some apparently yield at once to the eroding action of water, while others afford a resistance to this operation for reasons as yet unknown, but which are probably rather mechanical or physical than chemical. Knowing as we do so little of the conditions under which the different layers of limestone, almost or quite identical in composition, were formed, we can only speculate that those layers which resisted erosion were more compact, hard, and dense, perhaps more metamorphosed by a subsequent crystallization than the others, while we actually have no facts on which to base such theories. No better illustration of the darkness amidst which geologists are seeking light can be given than by stating that we are in complete ignorance of the causes which produce different layers of limestone, almost identical in composition, the one above the other. We can explain alternations of shale, sandstone and limestone by changes in depth of the sea in which they were formed; but such an explanation does not hold good where the

same rock continued to be formed. Why should the sediment, whether chemical or mechanical, have formed a continuous layer an inch to several feet in thickness, and then a break in continuity have occurred, to be succeeded by another layer of the same material?

While the greater portion of the limestone has in all probability been formed in deep water, we have one instance in a quarry at Uhlersville on the Delaware where it must have been formed as a beach, since we find here distinct traces of ripple marks along the entire face of the quarry, some sixty feet high and fifty feet deep, the strata being tilted nearly vertically.

It has been generally supposed that the limestone dips almost universally southward; and while this view holds good for Northampton county, except at the junction of No. II with the No. III slates and along the north flank of the South Mountains, it is not the case in Lehigh county; for here we find northwest dips, more especially along an axis which is prolonged some distance into Northampton county, a short distance above Catasauqua.

As a general thing the limestones pass conformably under the No. III slates, and the few exceptions where the slates dip toward the limestones, and the latter away from the slates can readily be explained by an overturning of the beds toward the south, by which means as in the slate quarry close to and south of Ironton the slate apparently passes conformably below the limestone.

Overlying the No. II limestone occurs the Trenton limestone which is more fossiliferous and contains such characteristic fossils as *Chætetes lycoperdon* and *Orthis pectinella* as well as the stems of an encrinite. It was first found about a mile south of Ironton in Lehigh county, then at intervals between Bath and Martins Creek in Northampton county; but all attempts to trace it as a continuous formation have thus far been unsuccessful owing to the lack of outcrops. It occurs most extensively at Martins Creek on the Delaware, at a point a little south of the cotton mill, and is there as elsewhere apparently conformable with the underlying Magnesian limestone.

This limestone resembles in appearance the No. II, being however more compact and not at all crystalline, and of a gray black color.

There has been no apparent sudden break between the two, but the transition has been a gradual one. This was to be expected if the subsidence of the sea-bottom was steady and slow. An examination of the beds between Ironton in Lehigh county and the Delaware River, as close to the junction of the limestone and slate as possible, has shown that the limestone for the entire distance is more or less a hydraulic one, due to the greater

proportion of alumina which it contains. This also was to be expected if the subsidence continued, as signaling an approach to the era of slate-formation and open-sea deposition. These limestones are utilized on the Lehigh river in the manufacture of hydraulic cements and lately Portland cement has been made at the Copley Cement Works, which is said to be nearly or quite equal to the imported. Careful search and the demand for it will no doubt cause this variety of the limestone to be explored at various other points in the two counties, and will in time render us independent of the cement now sent from the Hudson River. The limestone is of a dull, earthy appearance, entirely free from any crystalline texture and of a dark gray color.

Before closing our discussion of the limestone it is necessary to speak of the large and numerous deposits of brown hematite iron-ore which occur in it, and which form the main support of the extensive iron furnaces of the Lehigh and Schuylkill Valleys.

The brown hematite iron-ore occurs almost exclusively in two irregular lines of deposition; the one along the northern flank of the South Mountain Range, the other at or near the junction of the No. II limestone with the No. III slates. A few other localities, at which the ore is found, but these are insignificant in number compared to the two lines mentioned. Along both these lines the ore is always found either in hydromica slate, or resting on limestone very greatly impregnated with damourite; the same is true elsewhere whenever the brown hematite is found *in loco originali*. Some deposits are however found which have evidently been pockets or cavities in the limestone into which the masses of limonite have been forced together with gravel and clay during the Drift Period. Leaving these out of consideration as of minor importance, let us consider briefly those iron-ore deposits which occur in place. It is at once evident that like the rocks with which they are associated they are of secondary origin, and have been derived from still older formations. The occurrence of the brown hematites with silica, alumina, lime, magnesia and the alkalies, more especially potash, points to their having been derived from Archæan rocks containing orthoclase and either hornblende or pyroxene. From the decomposition of these three minerals we are able to derive all the oxides above mentioned including the iron which was without any doubt derived in great part from the decomposition of ferrous silicate present in the hornblende and pyroxene, while a portion of the iron may have been derived from iron pyrites, although this supposition is entirely unnecessary. It is extremely improbable that the brown hematite was derived from the peroxidation and hydration of magnetic iron ore, when we recall the great resistance

which the latter offers to chemical change of any kind when exposed to the action of air and water, and its unaltered condition and fresh, bright appearance in rivers and on the seashore. But the question as to how the brown hematite got into its present condition and whether it was deposited cotemporaneously with the rocks containing it, or subsequently to these, is still an enigma and various theories have been offered in explanation. For a resumé of some of these hypotheses reference may be made to a recent article by Professor J. D. Dana, in this Journal, III, vol. xiv, page 136. The almost entire freedom of the hydromica slate, when fresh, in Lehigh and Northampton counties from ferruginous minerals will prevent our having recourse to pyrite, pyrrhotite, chlorite, garnet, mica and staurolite, which Professor Dana says occur in the hydromica region of Connecticut. Hence we must have recourse to other sources. It seems most doubtful that the mineral, from which the brown hematites were derived, was deposited cotemporaneously with the hydromica slates in the district under discussion, since we find the ore often passing through the slate or clay obliquely and intersecting the bedding. It is more probable that the ore was conveyed to its present position by infiltration subsequent to the formation of the hydromica slates. Whence was it derived? I have already stated that the limestone contains varying proportion of ferrous carbonate and of pyrite, and when we consider the enormous erosion which the limestone has undergone, the wonder is not that the deposits of iron ore should be so great, but rather that they should be so small. The ferrous carbonate and the pyrite oxidised to ferrous sulphate being both soluble in water, the former when the water contains carbon dioxide, the waters would naturally carry these salts in solution until they came in contact with precipitating agents such as the alkaline silicates which the hydromica slates carry. These last became converted to carbonates and sulphates, leaving the iron behind, either directly as hydrated ferric oxide, or possibly as ferrous silicates which became later decomposed by the action of aerated water to hydrated ferric oxide and free silica, which latter we now find so universally associated with the brown hematites as quartz. Whatever the origin of these ores may have been, one thing is evident, viz., that there is some genetic relation between the brown hematites and the hydromica slates, as evidenced by the almost universal occurrence of the ore in the slate, extending all the way from Vermont to East Tennessee through the Great Valley as well as in the interior valleys of Pennsylvania where the No. II limestones occur.

It is well here to emphasize the fact that these brown hematite ores all belong to the Lower Silurian limestone formation,

since, in 1874, Dr. Sterry Hunt, after a cursory examination of Ziegler's Mine in Berks County, situated at the junction of the No. II limestone and the No. III slates, made the mistake, in a paper on "The Decay of Crystalline Rocks" before the National Academy of Science, of supposing that the hydromica slates belonged to the Huronian Period:—a mistake into which so eminent an observer as himself would never have fallen had he been better acquainted with the region.

At intervals along the junction of the limestones and slates there occurs a black carbonaceous shale, often decomposed to black or dark blue clay, which I have supposed to be the representative of the Utica shales. It consists of a very carbonaceous hydromica slate (containing damourite), without any fossils and may not belong to the Utica Period at all. In no instance has it been found more than one to twelve feet thick, but it sometimes carries pyrite from which a portion of the iron ores, just mentioned, may have been derived. These shales are of no economic importance.

Overlying these come the No. III, Hudson River or Matinal Slates, which extend into the Kittatinny Mountains. A large portion of these slates are extremely useful for roofing and other household purposes, and extensive quarries have been opened at various points for the purpose of extracting them, as, however, they have been but very slightly examined, during the progress of the present Geological Survey of the State, I shall defer a more detailed description of them to some future time.

ART. XXXIX.—*On the Influence of Temperature on the Optical Constants of Glass;* by CHARLES S. HASTINGS, of the Johns Hopkins University.

A FORMULA connecting the refractive power of a body with its density, established by Newton, is well known. This, though founded upon the assumption of the emission theory of light, is found to hold true for gases and vapors and, qualitatively at least, for most solids and liquids. Still various deviations from the law have long been recognized. Arago showed that water increased in refractive power in changing its temperature from that corresponding to its maximum density to the freezing point. This has since been made the subject of an investigation by Jamin, who gives 0° C. as the temperature of maximum refractive power.

More recently Dale and Gladstone have made an extensive study of the changes produced in the refractive and dispersive powers of various liquids by increase of temperature; a study

which has been largely extended by the labors of Schrauff, Landolt and others. In all liquids, with the exception noted above, increase of refractive power accompanies increase of density.

Observations of the same sort upon solid bodies lead, however, to much less accordant results. Rudberg, in investigating the optical properties of several crystals, found that in arragonite and quartz, the variations in density and refractive index are in the same direction, but that with calcite the case is different, the index for the ordinary ray seeming independent of changes in temperature, and for the extraordinary ray the variation is abnormal. This is less striking, however, when we bear in mind the discovery of Mitscherlich that the deportment of Iceland spar in regard to extension by increase of temperature is quite exceptional.

But before Rudberg's investigation, Arago and Neumann had independently pointed out the fact that glass does not follow the ordinary law. Fizeau undertook to determine quantitatively the changes which various glasses undergo, as regards their mean indices of refraction, and devised for that purpose, an elegant and celebrated method. I shall quote later, some of his results, the only quantitative ones which I have been able to find.

The instrument with which the following determinations were made, is the large spectrometer by Meyerstein, belonging to the Physical Laboratory of the Johns Hopkins University. The circle is 12 inches in diameter, divided to 6′, and reads by two microscopes to 2″. The probable error of one division is 1″·48, and its larger periodic errors are expressed in the formula

$$N = 2''\cdot386 + 7''\cdot82 \sin(z + 62^\circ\,36') + 2''.1 \sin(2z + 157^\circ) + 4''\cdot32 \sin(3z + 323^\circ) + 0''\cdot46 \sin(4z + 122^\circ)$$

N being the corrected and z the immediate reading.

Much labor was expended in putting the instrument in so satisfactory a state, for in order to secure uniformity of reading it was found necessary to re-grind the axis. The collimating telescope too, was, by inexcusable carelessness in construction, directed nearly one-fourth of an inch away from the axis, thus vitiating all determinations of double deviations by the introduction of unknown errors of aberration in the object glass.

At the center of the instrument is a small circular platform bearing a graduated silver limb at its edge which is read by two verniers to single minutes of arc. This platform turns independently of the large circle with inconsiderable friction, and upon it is placed the prism to be studied. The methods adopted to adjust the instrument and prism, and to measure the angle of the prism were exceedingly accurate and perhaps find a proper place for description here.

In the focal plane of the positive eye-piece was a reticle consisting of lines on glass ruled as follows:—a system of two pairs of parallel lines crossing at center of field, one pair being horizontal and the other vertical, the angular distance (measured from objective) between the components of each pair being about 1′; and a single line, vertical and about 25′ to the right of the vertical pair. Over this single line was placed a small totally reflecting prism, one of its faces turned toward an opening in the eye-tube. The advantage of this arrangement is at once evident, for it was possible to get a strongly illuminated image of the single line, reflected by a plane beyond the objective, between the two vertical lines, at the same time avoiding the annoyance of waste light reflected from the eye-lenses on the one hand, since the mirror was within the eye-piece, and from the objective on the other, owing to the eccentric position of the mirror.

The instrument was put in adjustment by means of a piece of plane-parallel glass placed vertically upon the platform, i. e., so that its plane was parallel to the axis of the instrument. The platform was then turned with the glass till the reflected image of the reticle was seen in the telescope. The telescope was then sharply focused upon this image and the eye-piece clamped in that position as evidently corresponding to adjustment for parallel rays, at least for rays of medium wave length. Then the glass was turned 180°. If the image of the horizontal lines corresponded with the lines themselves it was good; if not, the correction was divided between the glass and the telescope. Proceeding in this way, it was easy to adjust the telescope so that once sighting the reflected image it would be again visible in the same place by turning the glass 180°, and that independently of the azimuth of the platform as referred to the large circle. When these conditions are met it is clear that the axes of the instrument and platform are parallel, while that of the telescope is perpendicular to both. The proper adjustment of the collimating telescope as regards focus and direction, is so evidently attainable from these that it is not worth while describing it specially.

In order to measure the angle of a prism, it was placed upon the platform in the place of the glass plate and adjusted by the proper screws until both faces were perpendicular to the line of collimation of the telescope; a reading was then made when the image of the single vertical line fell exactly half way between the two wires at the center of the field. The error of setting was found to be generally less than 1″, not a surprising result when one remembers that the three lines were of the same intensity, and that a motion of but 30″ in the prism would carry the reflected line from one of the pair to the other.

The next step was to rotate the great circle, and with it the prism, until the other face took exactly the same position; the reading then would clearly be the supplement of the desired angle; in practice, however, the angle was repeated by turning back the platform independently of the circle, the repetition being carried usually to six times, and then back over the same ground for verification. This method was well adapted to this work as the mass moved independently was small, and turned with little friction. It failed however, as might be expected, in measuring later the angles of deviation, for here the joints to be turned were much larger, and that limit of accuracy which is always reached so easily and quickly by the method of repetition, was not below that of a single reading of the microscopes.

Of the adjustment of the prism for measuring the double angle of deviation for the different Fraunhofer lines, little need be said except that, besides the care taken in placing the surfaces exactly parallel to the axis of the instrument, the prism was shifted on the platform until, seen through the lens of the collimator by looking through the slightly opened slit, exactly the same central portion of the objective was projected upon the faces of the prism in both its right and left positions of minimum deviation. This is a very important precaution in order to eliminate errors due to aberration, which must exist in every lens save for rays of a single wave length, or, in rare cases, of two wave lengths.

The position of the prism for minimum deviation of a single line was carefully determined on the small circle attached to the platform, by experiment, while those of the other lines observed were computed after an approximate measurement of the angle of deviation.

The process followed in making the measurements, was to point the telescope upon the particular Fraunhofer line which was to be determined, with the prism in its first position of minimum deviation and read the circle by both microscopes, then to turn the prism to its second position of minimum deviation and read again, the difference of readings being the double deviation required. With this observation was also put the temperature of the prism at the time. This course was pursued from six to ten times, using in each case a different portion of the circle, and then a new line was undertaken. In order to determine the temperature of the prism, a metal cell was cemented to the prism, the back of which formed one side of the receptacle; in this was placed a small centigrade thermometer reading to quarter degrees, estimated, however, to tenths, and then filled with water. The reading of the barometer was also taken at each series of observations. It is needless to say that each reading of the circle was corrected by an

amount determined from the formula given above. The prisms were as follows:

I, Of Feil's flint glass, No. 1237, sp. gr., 3·554.
Angle of prism, 60° 4′ 56″·21±0″·18.
II, Of Feil's flint glass, No. 1241, sp. gr., 3·151.
Angle of prism, 59° 58′ 31″·25±0″·61.
III, Of Feil's crown glass, No. 1219, sp. gr., 2·482.
Angle of prism, 60° 10′ 9″·04±0″·23.
IV, Flint glass, sp. gr., 3·497.
Angle of prism, 50° 7′ 53″·01±0″·20.
V, Crown glass, sp. gr., 2·510.
Angle of prism, 59° 48′ 13″·40±0″·08.

The materials of IV and V have been in my possession a long time, and there is no way to determine what they are except from their optical properties, and the statement of the optician from whom I procured them, that the first is of French and the second of English manufacture. I suppose IV to be a flint glass of Feil's making, while V is doubtless of the so-called "soft crown" of Chance Brothers.

The temperature affecting the observations ranges from 14°·9 C. to 29° C., though most of the lines were measured with a maximum difference of 10° C.

The reduction of the observations on the first three prisms to a temperature of 20° C. and barometric height of thirty inches gave the following indices of refraction:

Temperature, 20° C. Barometric height, 30 inches.

	I.		II.		III.	
Line.	n	e	n	e	n	e
A	1·615258	±3	1·572464	±6	1·512456	±3
B	1·618706	3	1·575332	7	1·514369	3
C	1·620482	3	1·576815	6	1·515334	3
D_2	1·625421	3	1·580905	6	1·517965	3
5614	1·628001	2	1·583024	6	1·519292	3
E	1·631893	2	1·586214	7	1·521274	3
F	1·637756	2	1·590989	6	1·524182	3
4548	1·643524	3	1·595660	7	1·526981	3
G	1·649086	3	1·600129	7	1·529597	3
h	1·654848	3	1·604749	7	1·532251	3
3951	1·659757	2	1·608658	7	1·534458	3

In this table the first column indicates the Fraunhofer line to which the indices of refraction, in columns headed n, refer. The last line is beautifully defined and lies nearly midway between H_1 and H_2. The columns under e give the probable errors of each determination in units of the sixth place decimal. It should be remarked here that the larger probable errors in the determinations of prism II arise from the greater probable error in the determination of its refracting angle, hence the

values are relatively, though not absolutely, as accurate as the others.

The following table contains, in the columns under k', increase of refractive index also in units of sixth place, corresponding to an increase of 1° C. in temperature.

	I.		II.		III.	
Line.	k'	k	k'	k	k'	k
A	4·12	3·79	1·70	1·74	− ·82	− ·77
B	4·04	4·22	2·35	2·04	− ·30	− ·53
C	3·96	4·45	2·77	2·19	− ·06	− ·41
D_2	5·12	5·07	2·43	2·61	− ·00	− ·08
5614	6·58	5·40	2·56	2·84	+ ·04	+ ·10
E	5·42	5·87	2·68	3·16		+ ·36
F	5·69	6·57	3·52	3·63	− ·09	+ ·74
4548		7·24	4·32	4·09		+1·11
G	8·53	7·85	3·86	4·51	+1·26	+1·44
h	8·03	8·48	4·78	4·93	+1·05	+1·78
3951	9·21	8·99	6·04	5·28	+3·22	+2·06

It is evident at a glance that the values of k' increase in each case with the decrease of wave length; and it was found that the quantities could be embodied, within the limits of error of the observations, by a formula of the form

$$k=\alpha+\beta\left(\frac{1}{\lambda}\right)^2.$$

The significance of the constants α and β is clearly that the first is the change in refractive power for light of indefinitely great wave length, or briefly, the change in refractive power, and the second, the change in dispersive power. The following are the numerical values for the three cases:

$$\text{I}.....k= 1{\cdot}875+1{\cdot}110\frac{1}{\lambda^2}.$$

$$\text{II}.....k= \cdot 442+ \cdot 755\frac{1}{\lambda^2}.$$

$$\text{III}.....k=-1{\cdot}813+0{\cdot}604\frac{1}{\lambda^2}.$$

From these, the quantities contained in the columns headed k are computed.

The most surprising fact which these results point out is, that the variation in dispersive power attending variation in temperature is relatively enormously greater than that of the refractive power. This has, I believe, escaped attention heretofore. It could hardly have passed unheeded, however, did not a singular relation obtain in the coefficients. The dispersive powers of the three specimens of glass, computed in the ordinary way, are as 9 : 8 : 6 nearly, while the coefficients in

question are as 9 : 6 : 5 nearly; hence if this relation holds approximately for all optical glasses, as is probable, an achromatic combination good for one temperature is good for all others within moderate limits.

Fizeau found, in the researches before alluded to, values for the refractive increment for 1° C., not unlike our own, namely:

Crown glass (zinc), sp. gr., 2·626, k_D=0·00.
Common flint glass, sp. gr., 3·584, k_D=2·6.
Dense flint glass, sp. gr., 4·14, k_D=6·87.

The magnitudes of the quantities k show at once the importance of observing the temperature of the prism in every accurate determination of refractive indices, neglect in so doing generally vitiating the fifth decimal place. It may be remarked, too, that ordinary variations in barometric pressure cannot be neglected when it is desired to limit the errors to the sixth decimal place.

The prisms IV and V were not studied to determine the temperature constants as were the others, but were observed at as nearly the temperature of 20° C. as possible, and reduced by means of the constants given above. I add these results here because the indices are given with great accuracy, and the "soft crown," though largely used in photographic instruments, has not been much studied. It should be stated, however, that Van der Willigen has published the constants of a glass which seems nearly identical.

Temperature, 20° *C. Barometer*, 30 *inches.*

Line.	IV.	V.
A	1·603945	1·509607
B	1·607306	1·511584
C	1·609041	1·512580
D_2	1·613843	1·515288
5614	1·616333	1·516673
E	1·620103	1·518719
F	1·625751	1·521696
G	1·636702	1·527300
h	1·642284	1·530075
3951	1·647048	1·532381

The probable errors were not computed, for the reason that the temperature corrections were assumed; but as regards their accuracy, the values under IV are perhaps quite as good as those which go before; those under V, on the other hand, have much larger errors, though probably in every case below two units in the fifth place. This finds its explanation in the fact that the material was not homogeneous, thus giving less perfect definition than the others, though the surfaces of the prism were of the same excellence.

I have made these determinations for the purpose of discussing the theory of the astronomical objective.

Johns Hopkins University, January, 1878.

ART. XL.—*A note on Experiments with floating Magnets; showing the motions and arrangements in a plane of freely moving bodies, acted on by forces of attraction and repulsion; and serving in the study of the directions and motions of the lines of magnetic force;* by ALFRED M. MAYER.

FOR one of my little books of the Experimental Science Series I have devised a system of experiments which illustrate the action of atomic forces, and the atomic arrangement in molecules, in so pleasing a manner, that I think these experiments should be known to those interested in the study and teaching of physics.

A dozen or more of No. 5 or 6 sewing needles are magnetized with their points of the same polarity, say north. Each needle is run into a small cork, $\frac{1}{4}$ in long and $\frac{3}{16}$ in. in diameter, which is of such size that it just floats the needle in an upright position. The eye end of the needle just comes through the top of the cork.

Float three of these vertical magnetic needles in a bowl of water, and then slowly bring down over them the N. pole of a rather large cylindrical magnet. The mutually repellant needles at once approach each other and finally arrange themselves at the vertices of an equilateral triangle, thus ∴. The needles come nearer together or go further away as the magnet, above them, approaches them or is removed from them. Vibrations of the magnet up and down cause the needles to vibrate; the triangle formed by them alternately increasing and diminishing in size.

On lifting the magnet vertically to a distance the needles mutually repel and end by taking up positions at the vertices of a triangle inscribed to the bowl.

Four floating needles take these two forms

Five " " " " " "

Six " " " " " "

Seven " " " " " "

I have obtained the figures up to the combination of twenty floating needles. Some of these forms are stable; others are unstable, and are sent into the stable forms by vibration.

These experiments can be varied without end. It is certainly interesting to see the mutual effect of two or more

vibrating systems, each ruled more or less by the motions of its own superposed magnet; to witness the deformations and decompositions of one molecular arrangement by the vibrations of a neighboring group, to note the changes in form which take place when a larger magnet enters the combination, and to see the deformation of groups produced by the side action of a magnet placed near the bowl.

In the vertical lantern these exhibitions are suggestive of much thought to the student. Of course they are merely suggestions and illustrations of molecular actions and forms; for they exhibit only the results of actions in a plane; so the student should be careful how he draws conclusions from them as to the grouping and mutual actions of molecules in space.

I will here add that I use needles floating vertically and horizontally in water as delicate and mobile indicators of magnetic actions; such as the determination of the position of the poles in magnets, and the displacement of the lines of magnetic force during inductive action on plates of metal, at rest and in motion.

The vibratory motions in the lines of force in the Bell-telephone have been studied from the motions of a needle (floating vertically under the pole of the magnet), caused by moving to and fro through determined distances, the thin iron plate in front of this magnet. These experiments are worth repeating by those who desire clearer conceptions of the manner of action of that remarkable instrument.

ART. XLI.—*On the Intrusive Nature of the Triassic Trap Sheets of New Jersey;* by I. C. RUSSELL.

ALTHOUGH the trap sheets which traverse the Triassic rocks of New Jersey and of the Connecticut Valley are commonly spoken of as being dikes of igneous rocks, yet the proof of their intrusive nature is seldom given. As the igneous origin of these rocks has been questioned by some persons, we take the present opportunity of calling the attention of geologists to a locality where the proof that these sheets of trap were really forced out in a molten condition between the layers of sedimentary rocks, is very positively shown.

The trap ridges of New Jersey have a general north and south direction, usually conformable with the strike of the associated sandstones and shales which compose the great mass of the Triassic formation. The trap rocks, also, seem usually to be conformable in dip with the stratified rocks above and below them. These facts, together with the consideration of

the rare occurrence of the exposure of the junction of the trap rocks with the stratified rocks that overlie them—owing to the removal of the latter by denudation, and to the line of contact being covered with drift or overgrown by vegetation—have led to the supposition that the sheets of trap were not intrusive, but were formed cotemporaneously with the shales and sandstones as a bed or stratum of igneous rock, which was spread out in a molten condition at the bottom of the shallow sea in which the stratified rocks were being deposited. The question in hand, then, is to determine (1) whether the plutonic rocks of the Triassic were spread out as a sheet of molten matter and allowed to cool and consolidate before the rocks that rest upon them were deposited, both, therefore, belonging to the same geological period; or (2) were the trap rocks forced out in a fused state among the sedimentary strata after their consolidation, which, consequently, would make them more recent than either the rocks above or below them.

For the purpose of deciding these questions to our own satisfaction, we have examined the trap ridge known as the First Newark Mountain, for some twenty miles of its course in the neighborhood of Plainfield, New Jersey. We hoped by making the following examinations to acquire the desired information in reference to the history of these mountains:

1st. To discover if the sedimentary rocks that repose upon the igneous ones have been changed from their normal condition by the action of heat at the surface of contact.

2d. To determine, if possible, if the trap sheets seem in all cases to be conformable in bedding with the stratified rocks with which they are associated.

It is not difficult to find the junction of these igneous rocks with the shales and sandstones that underlie them. In all such cases that have come under our notice, the stratified rocks have been found to be highly altered and show very plainly that they have been exposed to intense heat. At a number of places beneath the trap rock forming the Palisades on the western shore of the Hudson, this change may be observed; in some instances the sandstone beneath the trap has been metamorphosed into a compact vitreous quartzite. At the Falls of the Passaic the junction of the igneous rocks, with the altered sedimentary rocks beneath, is well exposed. We have also observed similar indications of the action of great heat at a number of places in the shales and sandstones beneath the trap rocks in the neighborhood of Plainfield, New Jersey.

These observations indicate very clearly that the Triassic traps were once in a highly heated and probably molten condition, this is also shown by their crystalline structure. If these fused rocks had been allowed to cool and consolidate, as we

were at one time inclined to suppose, before the sandstones and shales above them were deposited, then, of course, the rock resting upon the trap would show no alteration similar to the changes that have affected the stratified rocks beneath.

As we have said, to obtain definite proof of such alteration in the stratified rock above the trap is quite difficult. Although the writer has made himself tolerably familiar with the Triassic formation of New Jersey, and has made many long excursions with the hope of finding such an exposure, yet in only one instance had he been successful. On the western slope of the First Newark Mountain, directly west of Westfield, New Jersey, and near the little deserted village of Feltville, the desired junction is very plainly shown. We there found one page of the history of the Triassic formation clearly legible.

At this locality the stratified rocks are well exposed in the sides of a deep ravine which has been carved out by a small brook that flows down the western slope of the mountain. The stream found but little difficulty in deepening its channel until it came to the hard igneous rocks over which it now flows. The trap rock, which appears in the bed of the stream in some places, presents its usual characteristics of a hard, bluish, crystalline rock, with a conchoidal fracture, similar in every way with the appearance it presents in neighboring localities where it is more thoroughly exposed. In other places it swells up into bosses and rounded masses which are thrust up into the overlying rocks. The outside of these masses present a scoriaceous or slag-like appearance; in the interior the cavities are filled with infiltrated minerals. The shales that rest directly upon these igneous rocks have, in many places, been disturbed from their normal position and greatly altered in texture and color. For the first two or three feet above the trap the shales have been intensely metamorphosed, and are scarcely to be distinguished from the trap itself. In hand specimens it is frequently impossible to determine from their appearance alone which is trap and which is metamorphosed shale. At a distance of six or eight feet above the trap the shales are still very much altered and filled with a great number of small spherical masses of a dark green mineral, resembling epidote. Midway up the sides of the ravine, which is about thirty feet deep, the shales present somewhat of their usual reddish appearance, but are traversed by a great number of irregular cavities formed by the expansion of vapor, while the rocks were in a semi-plastic condition. At a distance of twenty-five or thirty feet above the trap, the shales and sandstones are changed but slightly, if at all, from their normal condition. A bed of limestone from two to three feet in thickness, which is here interstratified with the shales and sandstones—a very rare occurrence in the Triassic

formation of New Jersey—where it approaches the trap is considerably altered and forms a mass of semi-crystallized carbonate of lime.

Near the junction of the metamorphosed shales and the igneous rocks beneath, we found in a number of places a peculiar rock composed of angular fragments of a greenish rock, resembling dark-green serpentine in appearance, bound together by a reddish cement—thus forming a typical breccia. This remarkable rock, which we have never seen in any other locality, in some places is two feet or more in thickness, at other times it fills the spaces between concentric masses of igneous rock or metamorphosed shale. Numerous small cavities that occur in the breccia have been filled by infiltration with calcite and zeolites. This interesting material seems to have a history somewhat similar to that of the "friction breccias," mentioned by Von Cotta, as occurring at the margins of eruptive igneous rocks, and formed at the time of their eruption.

The section at Feltville furnishes indisputable evidence that the igneous rocks, composing the First Newark Mountain, were intruded in a molten state between the layers of the stratified rocks subsequent to their consolidation. As these mountains are in every way similar to the rest of the trap ridges which traverse the Triassic regions of New Jersey, from analogy we should extend this conclusion so as to embrace them all.

The distinctness with which this one consideration relating to the Triassic trap sheets has been answered, seems by contrast only to make other questions in their history more obscure. We cannot now determine in what age after the consolidation of the Triassic sedimentary rocks the outbursts of trap occurred. Neither can we decide whether or not the several trap ridges that traverse the Triassic were formed at one time. It may be that one is thousands of years older than its neighbor.

As regards the conformability of the trap sheets with the associated sedimentary rocks, we have but little information to offer, independent of the section at Feltville which we have already described. The curved course which a number of the trap ridges in New Jersey follow, seems to indicate that they must cut across the strata of the sedimentary rocks, which, throughout the whole Triassic area in New Jersey, have a nearly uniform dip of from twelve degrees to fifteen degrees toward the northwest.

ART. XLII.—*Research on the Absolute Unit of Electrical Resistance;* by HENRY A. ROWLAND,* Professor of Physics in the Johns Hopkins University, Baltimore, Md.

Preliminary Remarks.

SINCE the classical determination of the absolute unit of electrical resistance by the Committee on Electrical Standards of the British Association, two re-determinations have been made, one in Germany and the other in Denmark, which each differ two per cent from the British Association determination, the one on one side and the other on the other side, making a total difference of four per cent between the two. Such a great difference in experiments which are capable of considerable exactness, seems so strange that I decided to make a new determination by a method different from any yet used, and which seemed capable of the greatest exactness; and to guard against all error, it was decided to determine all the important factors in at least two different ways, and to eliminate most of the corrections by the method of experiment, rather than by calculation. The method of experiment depended upon the induction of a current on a closed circuit, and in this respect resembled that of Kirchhoff, but it differed from his inasmuch as, in my experiment, the induction current was produced by reversing the main current, and in Kirchhoff's by removing the circuits to a distance from each other. And it seems to me that this method is capable of greater exactness than any other, and it certainly possessed the greatest simplicity in theory and facility in experiment.

In the carrying out of the experiment I have partly availed myself of my own instruments and have partly drawn on the collection of the University, which possesses many unique and accurate instruments for electric and magnetic measurements. To insure uniformity and accuracy, the coils of all these instruments have been wound with my own hands and the measurements reduced to a standard rule which was again compared with the standard at Washington. Unlike many German instruments, quite fine wire has always been used and the number of coils multiplied, for in this way the constants of the coils can be more exactly determined, there is less relative action from the wire connecting the coils, and above all we know exactly where the current passes.

The experiment was performed in the back room of a small house near the University, which was reasonably free from

* I am greatly indebted to Mr. Jacques, Fellow of the University, who is an excellent observer, for his assistance during the experiment, particularly in reading the tangent galvanometer.

magnetic and other physical disturbances. As the magnetic disturbance was eliminated in the experiment, it was not necessary to select a region entirely free from such disturbance. The small probable error proves that sufficient precaution was taken in this respect.

The result of the experiment that the British Association unit is too great by about ·88 per cent, agrees well with Joule's experiment on the heat generated in a wire by a current, and makes the mechanical equivalent as thus obtained very nearly that which he found from friction: it is intermediate between the result of Lorenz and the British Association Committee; and it agrees almost exactly with the British Association Committee's experiments, if we accept the correction which I have applied below.

The difference of nearly three per cent which remains between my result and that of Kohlrausch is difficult to explain, but it is thought that something has been done in this direction in the criticism of his method and results which are entered into below. My value, when introduced into Thomson's and Maxwell's values of the ratio of the electromagnetic to the electrostatic units of electricity, caused a yet further deviation from its value as given in Maxwell's electromagnetic theory of light: but experiments on this ratio have not yet attained the highest accuracy.

History.

The first determination of the resistance of a wire in absolute measure was made by Kirchhoff* in 1849 in answer to a question propounded by Neumann, in whose theory of electrodynamic induction a constant appeared whose numerical value was unknown until that time. His method, like that of this paper, depended on induction from currents: only one galvanometer was used and the primary current was measured by allowing only a small proportion of it to pass through the galvanometer by means of a shunt, while all the induced current passed through it. But, owing to the heating of the wires, the shunt ratio cannot be relied upon as constant, and hence the defect of the method. At present this experiment has only historical value, seeing that no exact record was kept of it in a standard resistance. However we know that the wire was of copper and the temperature 0° R and that the result obtained gave the resistance of the wire $\frac{1}{7}$ smaller than Weber found for the same wire at 20° R. in 1851.

In 1851, Weber published† experiments by two methods, first by means of an earth inductor, and second by observing

* Bestimmung der Constanten von welcher die Intensität inducirter elektrischer Ströme abhängt. Pogg. Ann, Bd. 76, S. 412.

† Elektrodynamische Maasbestimmungen; or Pogg. Ann., Bd. 82, S. 337.

the damping of a swinging needle. Three experiments gave for the resistance of the circuit $1903 \cdot 10^8$, $1898 \cdot 10^8$, and $1900\ 10^8$ $\frac{mm.}{sec.}$, but it is to be noted that a correction of five eighths per cent was made on account of the time, two seconds, which it took to turn the earth-inductor, and that no account was taken of the temperature, although the material was copper. He finds for the value of the Jacobi unit, $598 \cdot 10^7 \frac{mm.}{sec.}$. Three years after that, in 1853, Weber made another determination of the specific resistance of copper.* But these determinations were more to develop the method than for exact measurement, and it was not until 1862† that Weber made an exact determination which he expected to be standard. In this last determination he used a method compounded of his first two methods by which the constant of the galvanometer was eliminated, and the same method has since been used by Kohlrausch in his experiments of 1870. The results of these experiments were embodied in a determination of the value of the Siemens unit and of a standard which was sent by Sir Wm. Thomson. As the old Siemens units seem to vary among themselves one or two per cent, and as the result from Thomson's coil differs more than one per cent from that which would be obtained with any known value of the Siemens unit, we cannot be said to know the exact result of these experiments at the present time. Beside which, it was not until the experiments of Dr. Matthiessen on the electric permanence of metals and alloys, that a suitable material could be selected for the standard resistance.

The matter was in this state when a committee was appointed by the British Association in 1861, who, by their experiments which have extended through eight years, have done so much for the absolute system of electrical measurements. But the actual determination of the unit was made in 1863–4. The method used was that of the revolving coil of Sir William Thomson, the principal advantage of which was its simplicity and the fact that the local variation of the earth's magnetism was entirely eliminated and only entered into the calculation as a small correction. The principle of the method is of extreme beauty, seeing that the same earth's magnetism which causes the needle at the center of the coil to point in the magnetic meridian also causes the current in the revolving coil which deflects the needle from that meridian. Whenever a conducting body moves in a magnetic field, currents are generated in it in such direction that the total re-

* Abh. d. Kön. Ges. d. Wissenschaften zu Göttingen, Bd. 5.

† Zur Galvanometrie; Göttingen, 1862. Also Abh. d. K. Ges. d. Wis. zu Göttingen, Bd. 10.

sultant action is such that the lines of force are apparently dragged after the body as though they met with resistance in passing through it: and so we may regard Thomson's method as a means of measuring the amount of this dragging action.

But, however beautiful and apparently simple the method may appear in theory, yet when we come to the details we find many reasons for not expecting the finest results from it. Nearly all these reasons have been stated by Kohlrausch, and I can do barely more in this direction than review his objections, point out the direction in which each would affect the result, and perhaps in some cases estimate the amount.

In the first place, as the needle also induced currents in the coil which tended in turn to deflect the needle, the needle must have a very small magnetic moment in order that this term may be small enough to be treated as a correction. For this reason the magnetic needle was a small steel sphere 8 mm. diameter, and not magnetized to saturation. It is evident that in a quiescent magnetic field such a magnet would give the direction of the lines of force as accurately as the large magnets of Gauss and Weber, weighing many pounds. But the magnetic force due to the revolving coil is intermittent and the needle must show as it were the average force, together with the action due to induced magnetization. Whether the magnet shows the average force acting on it or not, depends upon the constancy of the magnetic axis, and there seems to be no reason to suppose that this would change in the slightest, though it would have been better to have made the form of the magnet such that it would have been impossible. The induced magnetism of the sphere would not affect the result, were it not for the time taken in magnetization: on this account the needle is dragged with the coil, and hence makes the deflection greater than it should be, and the absolute value of the Ohm too small by a very small quantity. The currents induced in the suspended parts also act in the same direction. Neither of these can be estimated, but they are evidently very minute.

The mere fact that this small magnet was attached to a comparatively large mirror which was exposed to air currents could hardly have affected the result, seeing that the disturbances would have been all eliminated except those due to air currents from the revolving coil, and which we are assured did not exist from the fact that no deflection took place when the coil was revolved with the circuit broken. In revolving the coil in opposite directions very different results were obtained, and the explanation of this has caused considerable discussion. As this is of fundamental importance I shall consider it in detail.

The magnet was suspended by a single fiber seven feet long,

and the deflection was diminished by its torsion ·00132. No mention is made of the method used for untwisting the fiber, and we see that it would require only 2·11 turns to deflect the needle 1° from the meridian. To estimate the approximate effect of this, we may omit from Maxwell's equation* all the other minor corrections and we have

$$R=\tfrac{1}{2}\frac{GKw\cos\varphi}{\sin\varphi+t(\varphi-\beta)}=\tfrac{1}{2}\frac{GKw}{\tan\varphi(1+t)}\frac{1}{\left(1-\frac{\beta t}{\sin\varphi(1+t)}\right)}\text{ nearly,}$$

where we have substituted $\varphi-\beta$ for φ in Maxwell's equation in the term involving t. In this equation φ is measured from the magnetic meridian; but let us take ψ as the angle from the point of equilibrium. Then $\psi'=\varphi'+\alpha$ and $\psi''=\varphi''-\alpha$, where ψ' and φ' are for negative rotation and ψ'' and φ'' for positive rotation and $\alpha=$ arc sin $\frac{\beta t}{1+t}$.

Let $$C=\frac{2}{GKw}.$$

Then $$CR'=\frac{1}{\tan\psi'(1+t)}.$$

$$CR''=\frac{1}{\tan\psi''(1+t)}.$$

$$R_{,}=\tfrac{1}{2}(R'+R'').$$

Where R′ and R″ are the apparent values of the resistance as calculated from the negative and positive rotations, and $R_{,}$ is the mean of the two as taken from the table published by the British Association Committee. If R is the true resistance,

$$CR=\frac{1}{\tan\varphi'(1+t)\left(1+\frac{\sin\alpha}{\sin\varphi'}\right)}=\frac{1}{\tan\varphi''(1+t)\left(1-\frac{\sin\alpha}{\sin\varphi''}\right)}.$$

We shall then find approximately

$$R=\frac{1+\tan\psi'\tan\alpha}{R'\left(1+\frac{\sin\alpha}{\sin\varphi'}\right)\left(1-\frac{\tan\alpha}{\tan\psi''}\right)}=\frac{1-\tan\psi''\tan\alpha}{R''\left(1-\frac{\sin\alpha}{\sin\varphi''}\right)\left(1+\frac{\tan\alpha}{\tan\psi''}\right)}.$$

When α is small compared with ψ'' or ψ', and when these are also small, we have

$$R=R_{,}(1+\alpha^2(\alpha^2-\tfrac{1}{2}\psi^2)+\&c.).$$

So that by taking the mean of positive and negative rotations, the effect of torsion is almost entirely eliminated. Now α is the angle by which the needle is deflected from the magnetic meridian by the torsion and its value is $\frac{1}{2\psi}\left(1-\frac{R'}{R''}\right)$ nearly,

* "Reports on Electrical Standards," p. 103.

when α is small, and this, in one or two of their experiments, exceeds unity or α exceeds 28°·6, which is absurd. Taking even one of the ordinary cases where $\frac{R_1}{R_2}=1{\cdot}02$ and ψ is about $\frac{1}{20}$, we have $\alpha=12°\cdot$ nearly, which is a value so large that it would surely have been noticed. Hence we may conclude that no reasonable amount of torsion in the silk fiber could have produced the difference in the results from positive and negative rotation, as has been stated by Mr. Fleming Jenkin in his "Report on the New Unit of Electrical Resistance."*

The greatest value which we can possibly assign to α which might have remained unnoticed is $\frac{1}{10}$, which would not have affected the experiment to any appreciable extent.

Another source of error which may produce the difference we are discussing is connected with the heavy metal frame of the apparatus, in which currents can be induced by the revolving coil. The coil passes so near the frame-work that the currents in it must be quite strong and produce considerable magnetic effect. Kohlrausch has pointed out the existence of these currents, but has failed to consider the theory of them. Now, from the fact that after any number of revolutions the number of lines of force passing through any part of the apparatus is the same as before, we immediately deduce the fact that, if Ohm's law be correct, the algebraical sum of the currents at every point in the frame is zero, and hence the average magnetic action on the needle zero. But although these currents can have no direct action, they can still act by modifying the current in the coil; for while the coil is nearing one of the supports the current in the coil is less than the normal amount, and while it is leaving it it is greater; and although the total current in the coil is the normal amount, yet it acts on the needle at a different angle. By changing the direction of rotation, the effect is nearly but not quite eliminated. The amount of the effect is evidently dependent upon the velocity of rotation and increases with it in some unknown proportion, and the residual effect is evidently in the direction of making the action on the needle too small and thus of increasing R. If these currents are the cause of the different values of R obtained with positive and negative rotation, we should find that, if we picked out those experiments in which this difference was the greatest, they should give a larger value of R than the others. Taking the mean of all the results† in which this dif-

* "Reports on Electrical Standards," London, 1873, p. 191.

† In the table published by the Committee the different columns do not agree, and I have thought it probable that the last two numbers in the next to the last column should read 1·0032 and 1·0065 instead of 1·0040 and ·9981, and in my discussion I have considered them to read thus.

ference is greater than one per cent, we find for the Ohm $1{\cdot}0033\ \frac{\text{earth quad.}}{\text{sec.}}$, and when it is less than one per cent, $\cdot 9966\ \frac{\text{earth quad.}}{\text{sec.}}$, which is in accordance with the theory, the average velocities being $\frac{100}{26}$ and $\frac{100}{43}$ nearly. But the individual observations have too great a probable error for an exact comparison.

But whatever the cause of the effect we are considering, the following method of correction must apply. The experiments show that R is a function of the velocity of rotation, and hence, by Taylor's theorem, the true resistance R_0 must be

$$R_0 = R(1 + Aw + Bw^2 + \&c.),$$

and when R is the mean of results with positive and negative rotations,

$$R_0 = R(1 + Bw^2 + Dw^4 + \&c.).$$

Supposing that all the terms can be omitted except the first two, and using the above results for large and small velocities, we find $R_0 = \cdot 9926\ \frac{\text{earth quad.}}{\text{sec.}}$. But if we reject the two results in which the difference of positive and negative rotations is over seven per cent, we find

$$R_0 = \cdot 9934\ \frac{\text{earth quad.}}{\text{sec.}}.$$

The rejection of all the higher powers of w renders the correction uncertain, but it at least shows that the Ohm is somewhat smaller than it was meant to be, which agrees with my experiments.

It is to be regretted that the details of these experiments have never been published, and so an exact estimate of their value can never be made. Indeed, we have no data for determining the value of the Ohm from the experiments of 1863. All we know is that, in the final result, the 1864 experiments had five times the weight of those of 1863, and that the two results differed ·16 per cent, but which was the larger is not stated. Now the table of results published in the report of the 1864 experiments contains many errors, some of which we can find out by comparison of the columns. The following corrections seem probable in the eleven experiments: No. 4, second column, read 4·6375 for 4·6275. No. 10, fourth and fifth columns, read 1·0032 and +0·32 in place of 1·0040 and +0·40. No. 11, fourth and fifth columns, read 1·0065 and +0·65 in place of 0·9981 and −0·19. Whether we make these corrections or not the mean value is entirely incompatible with the statement with respect to the 1863 experiments.

With the corrections the mean value of the 1864 experiments is 1 Ohm = 1·00071 $\frac{\text{earth quad.}}{\text{sec.}}$, and without them, using the fourth column, it is 1·00014. With the corrections the difference between fast and slow rotation is ·6 per cent.

In the year 1870 Professor F. Kohlrausch made a new determination of Siemen's unit in absolute measure, the method being one formed out of a combination of Weber's two methods of the earth inductor and of damping, by which the constant of the galvanometer was eliminated, and is the same as Weber used in his experiments of 1862. His formula for the resistance of the circuit, omitting small corrections, is

$$w = \frac{32 S^2 T^2 t_0 (\lambda - \lambda_0) AB}{\pi^2 K \quad (A^2 + B^2)^2} \text{ approximately,}$$

where S is the surface of the earth inductor, T is the horizontal intensity of the earth's magnetism, K the moment of inertia of the magnet, t_0 the time of vibration of the magnet, λ the logarithmic decrement, and A and B are the arcs in the method of recoil.

One of the principal criticisms I have to offer with respect to this method is the great number of quantities difficult to observe, which enter the equation as squares, cubes, or even fourth powers. Thus S^2 depends upon the fourth power of the radius of the earth inductor. Now this earth inductor was wound years before by W. Weber, and the mean radius determined from the length of wire and controlled by measuring the circumference of the layers. Now the wire was nearly 3·2 mm. diameter with its coating, and the outer and inner radii were 115· mm. and 142 mm. Hence the diameter of the wire occupied two per cent of the radius of the coil, making it uncertain to what point the radius should be measured. As the coil is wound, each winding sinks into the space between the two wires beneath, except at one spot where it must pass over the tops of the lower wires. The wire must also be wound in a helix. All these facts tend to diminish S and make its value as deduced from the length of the wire too large; and any kinks or irregularities in the wire tend in the same direction. And these errors must be large in an earth-inductor of such dimensions, where the wire is so large and many layers are piled on each other. If we admit an error of one-half a millimeter in the radius as determined in this way, it would diminish the value of S^2 1·4 per cent, and make Kohlrausch's result only ·6 per cent greater than the result of the British Association Committee.

Three other quantities, T, λ and K, are very hard to determine with accuracy, and yet T enters as a square. It is to

be noted that this earth-inductor is the same as that used by Weber in his experiment of 1862, and which also gave a larger value to the Ohm than those of the British Association Committee. *Indeed, the results with this inductor and by this method form the only cases where the absolute resistance of the Ohm has been found greater than that from the experiments of the British Association Committee.*

There seems to be a small one-sided error in A and B which Kohlrausch does not mention, but which Weber, in his old experiments of 1851, considered worthy of a ·6 per cent correction, and which would diminish $\frac{AB}{(A^2+B^2)^2}$ by 1·2 per cent. This is the error due to loss of time in turning the earth-inductor. As Kohlrausch's needle had a longer time of vibration than Weber's, the correction will be much smaller. In Weber's estimate the damping was not taken into account, and indeed it is impossible to do so with exactness. To get some idea of the value of the correction, however, we can assume that the current from the earth-inductor is uniform through a time t''', and the complete solution then depends on the elimination of nine quantities from ten complicated equations, and which can only be accomplished approximately. If γ is the true value of the angular velocity, as given to the needle by the earth-inductor, and γ is the velocity as deduced from the ordinary equation for the method of recoil, I find

$$\frac{\gamma}{\gamma_1}=1+\tfrac{1}{2^4}\left(\pi\frac{t}{T}\right)^2\frac{\left(1-9\left(\frac{\lambda}{\pi}\right)^2\right)(1+\varepsilon^{4\lambda})}{(1+\varepsilon^{2\lambda})^2}+\text{ \&c.},$$

where λ is the logarithmic decrement, ε the base of the natural system of logarithms, T the time of vibration of the needle, and t the time during which the uniform current from the earth-inductor flows. In the actual case, the current from the earth-inductor is nearly proportional to $\sin t$, and hence it will be more exact to substitute

$$4\left(\frac{t}{\pi}\right)^2\int_0^{\frac{1}{2}\pi} t\sin t\,dt=4\left(\frac{t}{\pi}\right)^2$$

in the place of t^2. The formula then becomes

$$\frac{\gamma}{\gamma_1}=1+\tfrac{1}{6}\left(\frac{t}{T}\right)^2\left(1-9\left(\frac{\lambda}{\pi}\right)^2\right)\frac{1+\varepsilon^{4\lambda}}{(1+\varepsilon^{2\lambda})^2}+\text{ \&c.}$$

This modification is more exact when λ is small than when it is large, but it is sufficiently exact in all cases to give some idea of the magnitude of the error to be feared from this source. Kohlrausch does not state how long it took him to turn his earth-inductor, but as T=34 seconds, we shall assume

$\frac{t}{T} = \frac{1}{\pi\sqrt{5}}$, and as $\lambda = \frac{1}{2}$ nearly, we have

$$\frac{\gamma}{\gamma_1} = 1{\cdot}0008,$$

which would diminish the value of the resistance by ·16 per cent.

As the time we have allowed for turning the earth-inductor is probably greater than it actually was, the actual correction will be less than this.

The correction for the extra current induced in the inductor and galvanometer, as given by Maxwell's equation,* has been shown by Stoletow to be too small to affect the result appreciably.

We may sum up our criticism of this experiment in a few words. The method is defective because, although absolute resistance has the dimensions of $\frac{\text{space}}{\text{time}}$, yet in this method the fourth power of space and the square of time enter, besides other quantities which are difficult to determine. The instruments are defective, because the earth-inductor was of such poor proportion and made of such large wire that its average radius was difficult to determine, and was undoubtedly overestimated.

It seems probable that a paper scale, which expands and contracts with the weather, was used. And lastly, the results with this inductor and by this method have twice given greater results than anybody else has ever found, and greater than the known values of the mechanical equivalent of heat would indicate.

The latest experiments on resistance have been made by Lorenz of Copenhagen,† by a new method of his own, or rather by an application of an experiment of Faraday's. It consists in measuring the difference of potential between the center and edge of a disc in rapid rotation in a field of known magnetic intensity.

A lengthy criticism of this experiment is not needed, seeing that it was made more to illustrate the method than to give a new value to the Ohm. The quantity primarily determined by the experiment was the absolute resistance of mercury, and the Ohm will have various values according to the different values which we assume for the resistance of mercury in Ohms.

One of the principal defects of the experiment is the large ratio between the radius of the revolving disc and the coil in which it revolved.

* "Electricity and Magnetism," Art. 762.
† Pogg. Ann., Bd. cxlix, (1873), p. 251.

In conclusion I give the following table of results, reduced as nearly as possible to the absolute value of the Ohm in $\frac{\text{earth quad.}}{\text{sec.}}$.*

Date.	Observer.	Value of Ohm.	Remarks.
1849	Kirchhoff	·88 to ·90	Approximately.
1851	Weber	·95 to ·97	"
1862	Weber	1·088	From Thomson's unit.
		1·075	From Weber's value of Siemen's unit.
1863–4	B. A. Committee	1·0000	Mean of all results.
		·993	Corrected to a zero velocity of coil.
1870	Kohlrausch	1·0196	
1873	Lorenz	·970	Taking ratio of quicksilver unit to Ohm =·962.
		·980	Taking ratio of quicksilver unit to Ohm =·953.
1876	Rowland	·9912	From a preliminary comparison with the B. A. unit.

[To be continued.]

ART. XLIII.—*On Croll's Hypothesis of the Origin of Solar and Sidereal Heat;* by Professor DANIEL KIRKWOOD.

THE Quarterly Journal of Science for July, 1877, contains an able and interesting article by James Croll, LL.D., F.R.S., on the age and origin of the sun's heat. The theory of Dr. Croll may be regarded as a compromise between the mathematicians, represented by Thomson, Tait and Newcomb, and the geologists of the uniformitarian school, represented by Playfair, Lyell, Darwin, etc. The principal points of this remarkable paper are:

1. That, as had been estimated by Sir William Thomson and others, but twenty million years' heat could be produced by the falling together of the sun's mass.

* Since this was written, a new determination has been made by H. F. Weber of Zurich, in which the different results agree with great accuracy. The result has been expressed in Siemen's units, and the comparison seems to have been made simply with a set of resistance coils and not with standards. The modern Siemen's units seem to be reasonably exact, but from the table published by the British Association Committee in 1864, it seems that at that time there was uncertainty as to its value. He obtains 1 S. U. = ·9550 $\frac{\text{earth quad.}}{\text{sec.}}$, which is greater or less than the British Association determination, according as we take the different ratios of the Siemen's to the British Association unit, ranging from ·14 per cent above to 1·92 per cent below. In any case the result agrees reasonably well with my own. The apparatus used does not seem to have been of the best, and the exact details are not given. But wooden coils to wind the wire on seem to have been used, which should immediately condemn the experiment where a pair of coils is used, seeing that in that case the constant, both of magnetic effect and of induction, depend on the distance of the coils. It is unfortunate that sufficient details are not given for me to enter into a criticism of the experiment.

2. That not less than five hundred millions of years have been required for the stratification of the earth's crust at the present rate of subaërial denudation; and hence that the gravitation theory of the origin of the sun's heat is incompatible with geological facts.

3. If we suppose two solid opaque bodies, each equal to half the sun's mass, to fall together in consequence simply of their mutual attraction, the collision would instantly generate sufficient heat to reduce the entire mass to a state of vapor. If, in addition to the motion resulting from their mutual attraction, we suppose the bodies to have had an original or independent motion towards each other of 202 miles per second, the concussion would produce 50,000,000 years' heat; a motion of 678 miles per second, together with that due to their mutual attraction, would generate 200,000,000 years' heat; and a velocity of 1,700 miles per second would generate an amount of heat which would keep up the supply at the present rate for 800,000,000 years.

4. The sun and all visible stars may have derived their heat from the collision of cold, opaque masses thus moving in space. The nebulæ are the products of the more recent impacts, and the stars have been formed by the condensation of ancient nebulæ.

5. This theory, while accepting the doctrine of the conservation of energy, indicates at the same time a possible supply of heat for several hundred millions of years; thus satisfying all moderate demands for geological time.

The mathematical correctness of the theory here stated will not be called in question. We shall consider merely the *probability* of the facts assumed as its basis. To the present writer the hypothesis seems unsatisfactory for the following reasons:

1. The existence of such sidereal bodies as the theory assumes is purely conjectural, unless it be claimed that lost or missing stars have become non-luminous, of which we have no conclusive evidence.

2. But, granting their existence, we have no instances of stellar motion comparable with those demanded by the hypothesis; the velocity being in most cases less than 50 miles, and in no case exceeding 200 miles per second.

3. If the two masses by whose collision the sun is supposed to have been formed were very unequal, as would be most probable, the amount of heat generated would be correspondingly less.

4. Such collisions as the theory assumes are wholly hypothetical. It is infinitely improbable that two cosmical bodies should move in the same straight line; and of two moving in different lines, it is improbable that either should impinge

against the other. Comets pass *round* the sun without collision against it.

5. But granting these difficulties removed, let us suppose that about 800,000,000 years ago two cold, opaque bodies, each containing one-half the matter of the solar system, were approaching one another in the same straight line, each at the rate of 1,700 miles per second;* that on meeting, their motion was transformed into heat; and that their united mass was at once reduced to vapor: the great question yet remains—How much of the period represented by these 800,000,000 years' heat can be claimed as *geological* time? The nebula formed by the collision would extend far beyond the present orbit of Neptune. The amount of heat radiated in a given time from so vast a surface would doubtless be much greater than that now emitted in an equal period. No considerable contraction could occur until a large proportion of the heat produced by the impact had been dissipated in space. It has been shown by Trowbridge† that with a temperature at the sun's surface of twice its present intensity the solar atmosphere would be expanded beyond the earth's orbit. The conclusion seems inevitable that much the greater part of the 800,000,000 years' heat must have been radiated into space before the planets were separated from the solar mass, and consequently, that the amount of geological time cannot, to any great extent, have exceeded the limits indicated by the researches of Sir William Thomson.

Upon the whole, it seems more difficult to grant the demands of Dr. Croll's hypothesis than to believe that in former ages the stratification of the earth's crust proceeded more rapidly than at present. The former, as we have seen, has no sufficient basis in the facts of observation. On the other hand, if our planet has cooled down from a state of igneous fluidity, the great heat of former times must doubtless have intensified both aqueous and atmospheric agencies in producing modifications of the earth's exterior.

Bloomington, Indiana, March, 1878.

* This is the greatest velocity mentioned by Dr. Croll. An increased rate of motion would, of course, produce more heat, but the hypothesis would be open to the same objections.

† Proc. Am. Phil. Soc., vol. xvi, pp. 329-333 and National Quarterly Review, March, 1877, p. 292.

ART. XLIV.—*On the chemical composition of Guanajuatite, or Selenide of Bismuth, from Guanajuato, Mexico;* by J. W. MALLET, University of Virginia.

THIS mineral seems to have been first noticed by Señor Castillo in March, 1873, and was by him partially described* as a sulpho-selenide of bismuth.

In the Guanajuato journal "La Republica" for July 13, 1873, Fernandez† published a full description, giving to the mineral the name Guanajuatite, and stating that it is solely a selenide of bismuth, a small amount of sulphur found being attributed to admixture with a little pyrite. In the same year or 1874 Rammelsberg‡ obtained as the result of a partial examination on a very small quantity,

Selenium	16·7
Bismuth	65·4
	82·1

and suggested the presence of zinc. The mineral was more fully examined by Frenzel,§ whose analysis yielded,

Selenium	24·13
Sulphur	6·60
Bismuth	67·38
	98·11

whence the formula has been deduced—$2Bi_2Se_3 . Bi_2S_3$.

In the 2d Appendix to the 5th edition of Dana's Mineralogy‖ the name Frenzelite was proposed for the species, but this has subsequently been retracted¶ in favor of the prior claim of the name Guanajuatite given by Fernandez.

The above are up to this time, I believe, the only published notices of the mineral in question. They leave two doubts in regard to its composition, namely, whether sulphur is really a constituent or only found from accidental admixture, and whether zinc is present or not.

At the Philadelphia Exhibition of 1876, my friend Señor Mariano Bárcena, of the Mexican Commission, was kind enough to give me authentic specimens of this mineral, partly in the original condition as found, and partly reduced to powder. I have availed myself of the opportunity thus afforded of at-

* Naturaleza, ii, 174 (1873); Jahrb. Min. (1874), 225.
† Quoted in this Journal, April, 1877, p. 319.
‡ 2d App. to 5th ed. Dana's Mineralogy (March, 1875), p. 22.
§ Jahrb. Min. (1874), 679.
‖ Loc. cit. ¶ This Journal, loc. cit.

tempting to settle the above questions by careful repetition of the chemical analysis. The already pulverized specimen was chiefly used, but was supplemented by a portion of the other —neither was altogether free from the hydrous silicate of aluminum which constitutes the gangue.

The method employed was the following. Water having been driven off by careful heating in a slow stream of carbon dioxide gas, collected and weighed, the mineral was mixed with ten times its weight of potassium cyanide and fused in an atmosphere of hydrogen. The mass on cooling was treated with water, and the solution filtered; the residue on the filter dried and again fused with the cyanide to ensure complete decomposition, repeating the treatment with water and filtration. From the mixed filtrates selenium was thrown down by addition of hydrochloric acid in excess, filtered after thirty-six hours on a weighed filter, cautiously dried and weighed; it was then burned, and a minute amount of silica left behind was determined. The solution from which the selenium had been precipitated was treated with potassium per-manganate as long as decolorization took place, and barium chloride then added; from the weight of barium sulphate thrown down sulphur was determined. The remaining solution was then evaporated to dryness at 100° C., the residue moistened with hydrochloric acid and treated with boiling water, leaving a further trace of silica; manganese (from the per-manganate used) and aluminum were now precipitated by ammonium sulphide, and separated by barium carbonate, the alumina being determined. The original residue of bismuth, left on the filter when the selenio-cyanate of potassium was filtered off, was dissolved in nitric acid, evaporated to dryness to separate a further portion of silica, redissolved, the bismuth thrown down by hydrosulphuric acid, filtered off, and a further portion of alumina (with a trace of ferric oxide) recovered from the filtrate. Lastly, the bismuth sulphide was carefully reduced by fusion with potassium cyanide, and weighed as metal.

The results were,

Selenium	31·64
Sulphur	·61
Bismuth	59·92
Alumina	2·53
Ferric oxide	*trace*
Silica	3·47
Water	1·46
	99·63

Zinc was specially looked for, both in the general analysis and using a separate portion for this purpose alone, but none could

be found. Possibly, as Rammelsberg had but a very small quantity of material on which to work, he may have been led to suspect the presence of zinc by a precipitate of aluminum hydrate derived from gangue.

No evidence, physical or chemical, could be found of the presence of pyrite; the trace (unweighable) of iron appears to belong to the gangue.

It is stated that this gangue is galapectite (Halloysite); if the amount of such mineral present be calculated from the alumina the above figures represent the specimen as composed of—

Guanajuatite		92·17
Gangue	Halloysite	6·72
	Quartz	·56
Moisture		·18
		99·63

and the Guanajuatite in the pure state would consist of

Selenium	34·33
Sulphur	·66
Bismuth	65·01
	100·00

Hence we have the atomic ratio,

$$Bi : Se : S = 310 : 432 : 21,$$

or, uniting the sulphur with selenium,

$$Bi : Se = 310 : 453 = 2{\cdot}000 : 2{\cdot}922,$$

a close approximation to 2 : 3, justifying of course the formula Bi_2Se_3.

The quantity of sulphur present is too small to warrant the assumption that it bears a simple atomic proportion to the selenium, but the former element certainly is present, and not as pyrite. One can scarcely suppose that in Frenzel's analysis nearly six per cent of iron was overlooked, as it must have been if pyrite were the cause (as suggested by Fernandez) of the occurrence of the sulphur found.

It seems clear that the mineral in question must be viewed as sesqui-selenide of bismuth, with isomorphous replacement to a variable extent of selenium by sulphur.

It is also mentioned (this Journal, April, 1867) that Fernandez has described a second selenide of bismuth from the same locality, and has derived from his analyses of more or less pure specimens the formula Bi_3Se. This formula is very improbable, since it involves the presence of an odd number of perissad atoms. Perhaps there may have been an admixture of native (metallic) bismuth.

ART. XLV.—*On the Janssen Solar Photograph and Optical Studies;* by S. P. LANGLEY.

Mr. JANSSEN, in papers lately presented to the French Institute* has given an account of recent results in solar-photography, obtained by him at the observatory of Meudon, and from the comments of Messrs. Huggins, Lockyer, De la Rue and other competent judges, it has been understood that remarkable advances have been made over any before produced. A copy has been published in the Annuaire du Bureau des Longitudes, but details cannot well be studied from such a print, on paper, and details are here all-important. As the writer has received, through the courtesy of M. Janssen, a fine positive on glass; the only one he knows of, as having yet reached this country, and as it not only bears important testimony to facts which have rested hitherto in part on statements in previous numbers of this Journal, but is in every way so remarkable as to constitute an important step in the history of solar physics, an account will be given of it here.

The means used by M. Janssen for producing the photograph are understood to be a telescope of about five or six inches aperture, and twelve feet focal length, embodying the improvements introduced by Mr. Rutherfurd, and others of M. Janssen's own, the most important change being a shortening the time of exposure to $\frac{1}{350}$ of a second, and at the same time an enlargement of the image. This involves a modification of the time of development, etc., and has evidently cost study and labor, the arduousness of which may be inferred from the perfection of the result.

As the photograph can only be made generally intelligible by some sort of illustration, the reader is referred to two papers in this Journal, the first in the number for February, 1874, where an article on the minute structure of the photosphere is illustrated by an Albertype plate, the second in March, 1875, accompanied by a steel engraving. The reader must also be detained briefly over a question of nomenclature which possibly has caused a misapprehension on Mr. Janssen's part, for the first paper undertook among other things, to specifically show that the elements of the photosphere were *not* "willow-leaf-" or "rice-grain-" like forms as had been commonly supposed. The term "rice-grain," it was carefully explained, was incorrect, and as an illustration imperfect. The name "rice-grain," for want of a generally accepted word, was used in the article, but under protest; especially as I had myself resolved these into minuter components, and carefully drawn specific instances

* Comptes Rendus, Oct. 29th and Dec. 31st, 1877.

of the foliate form and subdivision, specially calling attention to them in the plate, where they are found in two squares surrounded by a heavy outline. It is necessary to insist on the fact that purely optical methods had informed us of the nature of the constituents of the surface with a minuteness which photography has not even now attained. It was also stated in the first of these articles that the estimated mean distances between the centers of these composite objects ranged from 2″·57 to 1″·42 according to the degree of disintegration introduced by magnifying power, and the very important conclusion was reached that the light of the sun comes to us chiefly from an extremely small part of its surface—an indefinitely small part, but which is at any rate *less than one-fifth* of the whole. M. Janssen's impression that the true form and relative area of these has first been shown by the photograph is a misapprehension, though arising most naturally in part from the vicious nomenclature of the subject.

Until lately, photography has been useful, chiefly in fixing the positions and sizes of spots on the sun, rather than in studies of detail, which have hitherto been carried on with success only by the eye. The remarkable photograph before us begins a new order of things, for, though as we have said it does not reach absolutely all that the eye has yet caught, yet only those who have watched the sun with powerful instruments for years, can have enjoyed (and that rarely) the opportunity of seeing more than is here fixed for leisure study. It need hardly be said how immense is the gain of this opportunity for all to examine and verify deliberately; and it should be stated further that the photograph not only confirms previous results which have rested on the testimony of one or two individuals, but adds at least one important one of its own.

The plate before us is, as has been said, a positive, the solar image being 303^{mm}, or nearly twelve inches in diameter. The image viewed at a distance, shows the usual darkening toward the edge, though not quite uniformly all round, a circumstance probably not in the present case significant of any solar irregularity. Upon a closer view we see the coarse vague maculations or marblings* (formed as it seems to me by waves in the solar atmosphere, causing regions of greater thickness and consequently greater absorption by the heaping of the "rice grains"† and in some degree by their unequal distribution).

On a close approach we see the granular structure of the photosphere as it has never before been rendered in photography.

To give an idea of the precision of the photograph without the plate is hardly possible, but as the individual "grains"

* This is seen in the Albertype on removing it five or six yards from the eye where details are lost. The vagueness of the aggregations is in the original (i. e. in the sun itself.)

† Indicated all over the Albertype plates, and shown in specific details in the two designated squares.

may be here counted, I have placed on the positive a Rogers' reticule, consisting of very small squares, engine-divided on glass, which had been actually used on the sun for a similar purpose; and with its aid counted the "grains" in different parts of the plate not far from the center of the image, and taking the average I find a mean of about 5200 "grains" to the square inch, whence it appears, that at this rate, the plate actually exhibits within its circumference over half a million of these objects. From this measurement and the solar angular semi-diameter, it also appears that their mean distance in centers is 2″·2 which is in close agreement with optical determinations made with corresponding powers. As these objects are rather complex than simple, I venture the opinion that should M. Janssen succeed in future in enlarging his photograph while retaining his present wonderful definition, that their mean distance will tend to appear still less.

We are now brought to what is perhaps the most remarkable feature of the plate, for a continued examination shows what undoubtedly has not been established by optical studies, that there are extensive regions where the "grains" are distinctly seen, and others adjacent where they are confused and blurred as if by bad definition. There are numerous alternations of these areas of disturbance; which are themselves of varying sizes; perhaps it would be more correct to say that the general surface presents this blurry character, with small regions where the definition is as sharp and clear as we have described it. Now,—a question evidently to be asked,—is this bad definition something in the solar atmosphere or our own? Does it mean a tremendous disturbance over hundreds of millions of square leagues, or a quivering of the air a few yards from the camera?

First we may ask, how far has there been any anticipation of such local disturbance on the sun, away from the spots and away from the faculæ as seen on the edge? I believe there has been, from telescopic study, a somewhat uncertain recognition that the photospheric structure differed at different times, but nothing like the variations shown here was anticipated. Doubtless these alternations of structure in adjacent regions, if once recognized, would be visible to the telescope, if sought, and it has, in fact, often been asserted that the aspect of the granulations varied at different times from solar causes; but with the telescope we lack the facility for deliberate comparison of one part of the disc with another, we obtain here, since owing to the undulations which we do know without doubt, are in our own atmosphere, our best vision is but momentary, and before we can turn from one part of the sun to compare it with another the opportunity is gone. The photograph, obtained as it is, in less than a thousandth of a second, may be taken many times in succession, and reproduce more or less indefinite images, yet

the thousandth exposure may fall in the brief instant of definition the observer patiently watches for, and then the results of this rare moment are made permanent by the camera.

Curiously enough, these disturbances of the solar atmosphere have been more distinctly *felt* by the thermopile, perhaps, than seen by the telescope. In 1874, after summing the results of thermal measurements, carried on at Allegheny to determine the heat of different parts of the surface, and after eliminating the effects of increasing absorption toward the limb, of spots, etc., there remained irregularities of a small order which showed either a hitherto unrecognized solar phenomenon, or (what might conceivably be the case) some undetected causes of minute instrumental error. The difficulty of determining which was great, for it was a characteristic of these minute fluctuations, that they were neither both fixed in position or magnitude, but incessantly, changed place and degree. Only after a year's further study, I felt confident that I had so far eliminated the smaller instrumental errors, that I was in presence of residual phenomena, which, however minute, were real; and which corresponded to continual fluctuations in the depth of the solar atmosphere all over the surface, in the nature of local disturbances caused by its currents, and I felt justified in announcing the existence of these—which I had never seen—in positive terms.*

Nothing could be more unexpected than the confirmation of these statements which the photograph offers, but it would be most unjust to M. Janssen's eminently original and valuable work, not to state that such anticipations, however positive, are obscure compared with the light thrown on this part of the subject by his methods. We say this, under the reserves already hinted, as to the difficulty of distinguishing by a single plate, the exact limits between the effects of solar and telluric disturbance; but so much at least appears to be solar, that we have here something very like evidence of great waves, in the solar atmosphere, obscuring the photospheric structure, and not only obscuring but changing it; for the currents appear to have altered the shapes of the grains, and their disposition. M. Janssen seems to consider the ultimate form of these grains as literal spheres, but it appears to me that his own admirable photograph bears other evidence. The components of these "grains" when undisturbed do indeed tend to dome-like forms, but these have always appeared to me to be but the extremities of filaments; extremities which aggregated, cause the "grains," and which lifted higher than their fellows cause the faculæ; the filaments themselves, being seen a little, here and there on the surface where bent over, and seen at all their length in the spots. In other words, we may compare the pho-

* Comptes Rendus, Sept. 6, 1875, p. 438.

tosphere to a field of grain in which from a bird's-eye view, we see, in a calm, only the rounded summits of the wheat. Let a wind blow fitfully over the surface, bending the crests here and there and showing more of the form of the straws. This is, it seems to me, the suggested explanation of the elongated form of the "grains" shown in such an interesting manner in M. Janssen's photograph where the action of solar currents is indicated and accompanied with partial obscuration. Let a whirlwind beat down the grain, showing the stalks lying every way exposed throughout their length—these are the filaments in a spot.

Of course the simile is imperfect and is not to be carried further. I can only venture conjecture as to what these "grains" really are, but I have always believed and still believe that they are not mere globular or bubble-like forms, but are associated with something beneath the surface most probably connected with ascending and descending gaseous currents, which in some way bring to that surface the heat from the interior, and carry back to it the gases which have been cooled by radiation, perhaps even to the point where precipitation occurs.

As to the question of the real solar origin of certain of the less definite forms on the plate we may say then, that it is known that certain peculiarities in our own atmosphere tend to impress themselves on the photographic plate, along with the solar phenomena, and it would be doubtless desirable, if possible, that two such photographs as we have here should be taken at intervals of five or ten minutes apart to eliminate this. It has been frequently asked why this is not done. But those who ask such a question are not familiar with the rarity of the instants in which *such* photographs can be taken. Two indeed can be taken at any interval, but those like the one before us, demand not only the finest mechanical and chemical methods and still more the highest skill, but atmospheric conditions so brief as to rarely or never last during even the short time mentioned.

Finally, then, though without two photographs of equal excellence taken within a few minutes of each other, it is perhaps impracticable to say *exactly* how much of the inequality of the plate is solar, it seems possible to state from the intrinsic evidence of the plate itself, that on the main features of its most interesting evidence as to the action of solar storms we may rely, and in this statement the writer, distrustful of his own knowledge of photographic processes, has sought the opinion of the most competent judge in these matters before expressing his own.

Our conclusion is, that M. Janssen has accomplished a remarkable, indeed, a wonderful, advance in solar photography, and that his success is not only a brilliant, but a useful one, for which he should have the thanks of every student of solar physics.

Allegheny, Penn., March 14, 1878.

ART. XLVI.—*On the occurrence of a Tree-like fossil plant, Glyptodendron, in the Upper Silurian (Clinton) Rocks of Ohio;* by Professor E. W. CLAYPOLE, B.A., B.Sc. (London), of Antioch College, Yellow Springs, Ohio.

IN the month of July, 1877, while on a geological excursion in company with one of my students, Mr. Leven Siler, in the vicinity of Eaton, in Preble County, Ohio, the latter picked up and handed me a slab bearing the impression of a vegetable stem, which proved, on closer examination, to be that of a plant allied to Lepidodendron. As the beds in which we were working at the time lie at the very base of the "Clinton" of the Ohio Survey, and within a few feet of the break which marks the summit of the Cincinnati group of the Lower Silurian, the specimen immediately assumed unusual interest and importance; no *indisputable* traces of land-plants having then come to light from so low an horizon in America, and no remains of arborescent vegetation being known *with certainty* from strata of so old a date in the New or Old World.

The slab containing the impression was not taken out of the solid rock, but lay loose on the bank of Clinton limestone. This fact will naturally raise some question concerning its age in the mind of every geologist. Fortunately, however, we are not in this instance dependent upon such evidence. To any one practically familiar with the Clinton rocks as they crop out around the Cincinnati uplift no doubt can arise. The stone is a piece of yellow, rough, encrinital limestone, considerably weathered, with the characteristic appearance of the Clinton at Eaton and here. Moreover, by the side of the impression there lies embedded one of our commonest corals (Polyzoa?) closely resembling *Chætetes lycoperdon* (Hall) of the Clinton in New York, figured in the second volume of the New York Survey. Lastly, from the back of the slab I chipped out a small specimen of an *Illænus*; either *Illænus Daytonensis* (Hall and Whitfield, Ohio Pal., vol. ii, 1875), or *Illænus Barriensis* (Murch. and Hall, 1862) (i. g. *Illænus Ioxus* of Hall, 1847), the mould of which still remains in the slab. Its Silurian age is therefore placed beyond a doubt, no species of Illænus being known in America above the Niagara group.

In describing and naming it, my first thought was to place it in the genus Lepidodendron, as a provisional arrangement pending the discovery of more perfect specimens. But further study of the fossil and its nearest allies among the Sigillarids and Lepidodendrids has induced me to place it by itself in a new genus, which seems to form a connecting link between some other paleozoic genera. I append the following description:—

GLYPTODENDRON. Tree-like; stem cylindrical; surface marked with two parallel sets of ridges running spirally up the stem in opposite directions, crossing each other and thus forming rhomboidal areoles. Lower portion of areole depressed and probably representing or containing a leaf-scar. Depressed portion of areole (leaf-scar?) symmetrical (i. e. alike on the right and left sides.) Vascular scars, leaves, fruit, etc., unknown. The name is from the Greek γλύφω, I engrave, and alludes to the depressed areoles.

Glyptodendron Eatonense. Stem thick and trunk-like; the specimen from which this description was made measured when complete about six inches in diameter. Surface divided into rhomboidal areoles by two sets of narrow ridges parallel and equidistant, running spirally up the stem in opposite directions. These ridges cross each other nearly at right angles. The areoles thus formed measure about seven-sixteenths of an inch along each diagonal. Lower portion of areole deeply and evenly depressed and probably representing a sunken leaf-scar. Upper border of depressed portion rounded in outline and elevated, equalling in height the spiral ridges. No trace of the vascular scars can be seen in consequence of the roughness of the stone and the weathering it has undergone. Found near Eaton, Preble Co., Ohio.

Being anxious to have the opinion of some naturalist more experienced than myself in the subject of the Pre-carboniferous flora, I sent a drawing of the fossil to Dr. J. W. Dawson of Montreal, one of the best authorities on the subject on this continent. In his reply of Oct. 22, 1877, he expressed his conviction of the importance of the discovery, and, from an inspection of the drawing, suggested its resemblance to *L. tetragonum* St. or *Bergeria* of the Lower Carboniferous, and also to *Diplostegium*. He asked the full extent of the evidence of its age, and expressed a wish to see a cast before forming any opinion upon it. This was sent some time afterward, and in reply, Dr. Dawson has favored me with some notes upon it and with permission to use them here. He says, "The marks on your specimen, at first sight, resemble those of the *Lepidodendra* of the type of the *L. tetragonum* Sternb. (*Bergeria* of some authors), a very widely diffused type of Lower Carboniferous age, found about that horizon in Europe, America and Australia. They may, however, have belonged to a plant of the genus *Ulodendron* or *Lepidophloeus*, and since the vascular scars are not preserved, it is impossible to decide this question. It is further to be observed, that the areoles appear to be deeply depressed, being in this respect the reverse of the leaf-bases of *Lepidodendron*. It may possibly have belonged to a plant of the nature of a Tree-fern, or of a *Sigillaria* allied to *S. Menardi*, rather than to a true *Lepidoden-*

dron." "In speaking of the areoles, I take it for granted that the curvature of the cast represents that of the stem." "The specimen may, however, have been a bit of bark pressed out of shape."

My own opinion, after a careful examination of the original, is that the curvature of the cast *does* represent that of the stem, and consequently that Dr. Dawson's remarks on its resemblances are well founded. The bark of *Lepidodendra*, etc., when pressed as usually occurs in the Coal-measures, is constantly flattened. In a subsequent communication, Dr. Dawson alludes to the possibility suggested above, that the fossil may exhibit a composite character partaking of the character of more than one existing genus. The wide diffusion of the type which it most resembles in the Lower Carboniferous is good reason for believing that it is very ancient, and therefore its occurrence so low as the Clinton limestone is the less surprising.

In conclusion, I gladly express my indebtedness to Dr. J. W. Dawson, of Montreal, for valuable aid cheerfully rendered, and to Mr. Leo Lesquereux, of Columbus, in this State, for prompt and kind replies to letters of enquiry.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On certain Fundamental Thermo-chemical Data.*—Berthelot has re-determined with great care the heat data belonging to certain chemical reactions, using an improved calorimeter for the purpose, described in the memoir. The heat of formation of sulphurous oxide, although determined many times, was not up to this time accurately known. Thus sixteen grams of sulphur burned to gaseous sulphurous oxide gave Dulong 41·6 calories, Hess 41·1, Andrews 36·9, and Favre and Silbermann 35·6, the discrepancies being accounted for on the supposition that SO_3 was formed. In Berthelot's experiments the calorimeter was of thin glass, and the sulphur used the octahedral variety. The mean result was, for sixteen grams of sulphur, 34·55 calories. From this value, the author calculates the heat of formation of sulphuric acid and the sulphates, $S+O_2=SO_2$ for example (using the old atomic weights) evolving 51·8 calories, $SO+O_2=SO_3$ setting free 17·2, $S+O_2+HO=SO_3HO$ evolving 62·0, $S+O_4+H=SO_4H$, 96·5, $S+O_4+K=SO_4K$ (solid), 171·1, etc. The author also found the heat of combustion of carbonous oxide in his calorimeter to be 68·17 calories, for C_2 or twelve grams of carbon. Hence as C_2 as diamond, in burning to C_2O_4 evolves 94, C_2 in burning to C_2O_2 evolves $94-68·2=25·8$, or for amorphous carbon 28·8. Ethylene twenty-eight grams, yields 334·5 calories, acetylene twenty-six

grams, 317·5, and benzene seventy-eight grams, 776 calories. In the formation of the hydrogen compounds of bromine and iodine, H + Br gas = HBr gas yields 13·1 calories, while H + Br liquid = HBr gas gives 9·5, H + I gas = HI gas gives −0·9 calories, and H + I solid = HI gas gives −6·3. The heat of formation of bromic acid is −21·2, and of hypobromous acid −6·7.—*Ann. Chim. Phys.*, V, xiii, 1, Jan. 1878. G. F. B.

2. *Relations between the Atomic Weights of the Elements.*—WAECHTER has described certain additional relations between the atomic weights of certain of the elements and their properties. He formulates his conclusions as follows: 1st. The chemism of the elements tabulated, twenty-nine in number, diminishes from fluorine to silicon, and this with an increasing atomic weight and with an increasing valence; while from silicon to caesium the chemism also increases, the atomic weight also increasing but the valence diminishing. 2d. The arithmetical means of the atomic weights of pairs of elements of equally intense but of opposite attractions (i. e., one positive and the other negative) are nearly equal to one another, being about seventy-six. As for example, F and Cs, N and Di, Te and Mg, O and Ba, I and Na, Sb and Al. 3d. The fusing and the boiling points of the elements, so far as known, increase with increasing atomic weights and increasing valence, from fluorine to silicon; while from silicon to caesium, they diminish as the atomic weight rises and the valence falls. 4th. The specific heats of the elements given in the table, in the solid state, so far as known, diminish with increasing atomic weight and increasing valence. 5th. The specific gravity, in the solid state of the elements as given, is, with corresponding atomic weights, the greater, the higher the valence. 6th. The chemism of the negative metalloids—fluorine to silicon—for the metals proper, diminishes with increasing atomic weight and an increasing valence.—*Ber. Berl. Chem. Ges.*, xi, 11, Jan. 1878.

G. F. B.

3. *New Method for the Preparation of Nitrogen.*—GATEHOUSE has observed that when ammonium nitrate and manganese peroxide are heated together up to the fusing point of the former (about 180°) a violent reaction ensues, the mass becomes red hot and nitrogen is evolved. If the temperature be kept between 180° and 200°, the nitrogen is entirely pure, being formed according to the equation:

$$(NH_4NO_3)_4 + MnO_2 = Mn(NO_3)_2 + (H_2O)_8 + N_4.$$

In one experiment, three grams NH_4NO_3 heated with an equal weight of MnO_2 in a mercury bath kept at 205°, yielded 630 cubic centimeters of gas, nearly the theoretical quantity. If the temperature rises above 216° the manganous nitrate decomposes giving nitrous vapors and oxygen. At 221° the gas contained eight per cent of oxygen.—*Bull. Soc. Ch.*, II, xxix, 115, Feb. 1878. G. F. B.

4. *On a Method of separating Crystallized Silica, especially Quartz, from Silicates.*—LAUFER has suggested an improvement

upon Müller's process for separating the silica existing as quartz in rocks and soils from that combined in the admixed silicates, which appears to be satisfactory. It is founded on the well-known property of salt of phosphorus of decomposing silicates, dissolving the metallic oxides and setting the silica free, while at the same time the quartz present is not attacked. The finely pulverized material is weighed in a platinum crucible, sufficient salt of phosphorus to decompose the silicates is added, and the whole is carefully heated, first in an air bath, and finally in the blast till the mass is in quiet fusion. After cooling, the mass is detached, boiled with hydrochloric acid for a long time, filtered, and the residue extracted with boiling solution of soda to remove the silica coming from the silicates. The final residue is washed, dried, and weighed as quartz. On treating a gram of the quartz so obtained with hydrofluoric and sulphuric acids, only a few milligrams of sulphate was left. The errors of the method are only about 0·2 per cent.—*Ber. Berl. Chem. Ges.*, xi, 60, Jan. 1878. G. F. B.

5. *On the use of Stannous Chloride in the analysis of Nitro-compounds.*—Limpricht, noticing the facility with which an acid solution of stannous chloride reduced nitro-compounds, made a series of experiments to ascertain whether the reaction could not be made use of to determine their composition quantitatively, and with excellent success. When a weighed quantity of an organic nitro-compound is heated with a definite volume of stannous chloride solution of known strength, the nitro-group is converted into NH_2 thus: $NO_2+(SnCl_2)_3+(HCl)_6=NH_2+SnCl_4+(H_2O)_2$. By titrition the quantity of stannous chloride used is ascertained; and from this the amount of the NO_2 calculated. The solutions needed are: (1) Stannous chloride, prepared by dissolving 150 grams tin in concentrated hydrochloric acid, decanting, adding 50 c.c. of HCl and diluting to a liter; (2) soda solution, 180 grams anhydrous soda and 240 grams Seignette salt in a liter of water; (3) starch solution diluted and filtered; (4) iodine solution, 127 grams iodine and the necessary quantity of KI, in a liter. Of this tenth-normal solution, 1 c.c. =·0059 gr. Sn. =0·0007666 gr. NO_2; (5) Permanganate solution, made and titered as usual. In the analysis, 0·2 gram of the nitro-compound is weighed out, placed in a 100 c.c. flask with 10 c.c. of the tin solution, and warmed. After cooling the flask is filled to the mark with water, 10 c.c. is removed with a pipette, placed in a beaker, diluted, mixed with the soda solution until the precipitate at first produced is re-dissolved, and after adding some starch solution, titered with the iodine solution until the blue becomes permanent. The numerous results given show the process to be accurate.—*Ber. Berl. Chem. Ges.*, xi, 35, Jan. 1878. G. F. B.

6. *On a so-called Catalytic action of Carbon Disulphide.*—Hell and Mühlhäuser have observed a remarkable action of carbon disulphide in facilitating the action of bromine upon acetic and formic acids. While bromine and acetic acid, treated in a

water bath in sealed tubes for forty hours, have not the slightest action on each other, the addition of a small quantity of carbon disulphide to the mixture so facilitates the reaction that even in six hours it is completed, and in one case the tube exploded from the evolved hydrogen bromide, in two hours. Further investigation showed that the presence of the disulphide was not absolutely necessary, either for the formation of the addition or the substitution products of bromine and acetic acid, but that it facilitated the formation of both to an extraordinary degree, the time required for the action to be completed being in the exact inverse ratio of the amount of CS_2 present. The precise mode of action of the disulphide, the authors are now engaged in investigating. —*Ber. Berl. Chem. Ges.*, xi, 241, Feb. 1878. G. F. B.

7. *On the Conversion of Nitriles into Amides.*—PINNER and KLEIN showed a short time ago that any nitrile may be made to combine directly with any alcohol, by passing gaseous HCl or HBr into a mixture of the two. There is first formed the salt of an amido-chloride:

$$RCN+R'OH+(HCl)_2=RC\left\{\begin{matrix}NH_2\\OR'HCl,\\Cl\end{matrix}\right.$$

which immediately loses HCl and becomes a salt of an imide. Thus benzonitrile and isobutyl alcohol when thus treated give the hydrochlorate of benzimidobutyl ether, $C_6H_5C\left\{\begin{matrix}NH\\OC_4H_9\end{matrix}\right\}HCl$. By the action of alcoholic ammonia, the free benzimidobutyl ether $C_6H_5C\left\{\begin{matrix}NH\\OC_4H_9\end{matrix}\right.$ and benzimido-amide (or benzenylamimide) $C_6H_5C\left\{\begin{matrix}NH\\NH_2\end{matrix}\right\}HCl$ are obtained. The authors now describe benzimidoamide and its silver compound, and the action of ethyl iodide and of acetic oxide upon it, and also benzimidobutyl ether. Also a polymer of benzonitrile termed kyaphenin.—*Ber. Berl. Chem. Ges.*, xi, 4, Jan. 1878. G. F. B.

8. *Quercite a Pentacid Alcohol.*—HOMANN has published in full his memoir on quercite, giving the results of experiments made in Wislicenus's laboratory, which go to prove this sugar to be a pentacid alcohol. By the prolonged action of acetic oxide upon quercite in sealed tubes at 100° to 120°, a penta-acetate was obtained as an amorphous brittle mass, having the formula $C_6H_7(OC_2H_3O)_5$. Saponification with barium hydrate confirmed this composition. The tetra and the diacetate are also described. The pentanitrate or nitroquercite was obtained as a gummy mass, exploding on being heated. Acetyl chloride acts like acetic oxide. Fuming hydrochloric acid was without action.—*Liebig's Ann.*, cxc, 282, Jan. 1878. G. F. B.

9. *On the Acids of Cocoa Butter.*—KINGZETT has examined the chemical constitution of the Cocoa butter of commerce, having a fusing point of about 30° C. It was saponified by sodium hydrate, the sodium salt converted into a barium salt, this decom-

posed by HCl in presence of ether and the fatty acids obtained by distilling off the ether, were repeatedly crystallized from alcohol. By repeating the fractional crystallizations, there was obtained beside oleic acid, an acid of the formula $C_{12}H_{24}O_2$ and fusing point 57·5° as one extreme, and another $C_{44}H_{128}O_2$, of fusing point 72·2° as the other, both of which are new. To the latter the author gives the name *Theobromic acid.*—*J. Ch. Soc.*, xxxiii, 38, Jan. 1878. G. F. B.

10. *On a new Class of Acid Salts.*—Villiers has studied a new class of acid salts, the acid acetates, in which acetic acid itself plays the part of water of crystallization. Their general formula of the sodium salts is $C_2H_3NaO_2(C_2H_4O_2)\frac{n}{m}H_2O\frac{n'}{m'}$, where $\frac{n}{m}+\frac{n'}{m'}=2$ or 3. Thus, a salt crystallizing in small flat efflorescent prisms has the formula $C_2H_3NaO_2(C_2H_4O_2)\frac{4}{5}(H_2O)\frac{6}{5}$. Another of the second class, where $\frac{n}{m}+\frac{n'}{m'}=3$, is in small efflorescent prisms of the formula $C_2H_3NaO_2(C_2H_4O_2)\frac{1}{4}(H_2O)1\frac{1}{4}$.—*Bull. Soc. Ch.*, II, xxix, 153, Feb. 1878. G. F. B.

11. *Electro-Magnetic and Calorimetric Absolute Measurements.*—In the continuation of a paper with the above title, Professor H. F. Weber concludes that the value of the Siemen's mercury unit of resistance lies between $0{\cdot}9536 \times 10^{10}\left(\frac{\text{millim.}}{\text{sec.}}\right)$ and $0{\cdot}9550 \times 10^{10}\left(\frac{\text{millim.}}{\text{sec.}}\right)$ and that the value of the British unit is the value asserted, $10^{10}\left(\frac{\text{millim.}}{\text{sec.}}\right)$. Professor Weber also discusses the experiments of Favre on the quantities of heat developed by various electromotive forces in their circuits during the time in which they consume equal quantities of zinc. Favre asserted that "*the ratio of those quantities of heat gives quite another value than does the ratio of the corresponding electromotive forces when measured galvanometrically*," and finds that the conclusion is at variance with the galvanic laws of Joule, Ohm and Faraday. Weber from experiment shows that the ratio of the electromotive forces obtained by the heat method and the galvanic method is the same, and attributes the errors of Favre to the use of a mercury calorimeter, an instrument which Weber condemns. —*Phil. Mag.*, March 1878, No. 30, p. 189. J. T.

12. *Chemical Dynamics.*—Dr. C. R. Alder Wright and Mr. A. P. Luff of St. Mary's Hospital, London, have made a first report to the London Chemical Society* of their investigation on this subject. The chief point established by their paper is the general principle that "*that* reducing agent begins to act at the lowest temperature which has the greatest heat of combustion." Details of experiments are given on the reduction of cupric oxide and

* Journal of Chemical Society of London, January, 1878.

ferric oxide by carbonic oxide, by hydrogen and by carbon respectively; from which it appears that hydrogen acts at a lower temperature than carbon, and carbonic oxide at a lower temperature than hydrogen. The quantities of heat evolved by the combination of carbon, hydrogen, and carbonic oxide with sixteen grams of oxygen are taken as 47·78, 57·82 and 68·35 respectively. The effect of the different physical states both of the metallic oxides and of the charcoal used is likewise carefully discussed.

In connection with the above paper, Mr. M. M. Pattison Muir, of Caius College, Cambridge discusses the "influence exerted by time and mass in certain reactions in which insoluble salts are produced." The author following a previous suggestion of Gladstone has studied chiefly the reactions of potassic and sodic carbonates on calcic chloride in aqueous solution and finds 1, "that the greater portion of the chemical change is produced during the first five minutes of the action; after that time the action very much decreases in rapidity; the amount of change in the next twenty-five or thirty minutes is, however, always greater than the amount accomplished in the second period of the same duration. The action proceeds as it were with a rush at first; it then gradually becomes more and more slow." 2, "that the equation

$$CaCl_2 + M_2CO_3 = 2MCl + CaCO_3$$

does not furnish a full expression of the action of sodium or potassium carbonate upon calcium chloride. When the two salts are mixed in the proportion expressed by their respective formulæ, the action represented in the equation is not completed even after . . . forty-six hours. In order fully to realize the equation the mass of the alkaline carbonate must exceed that which would be actually required to transform the whole of the calcium chloride into carbonate were the equation strictly true. When two molecules of potassium carbonate are present to one molecule of potassium chloride the action is complete after the lapse of an hour; were sodium carbonate employed it would be necessary to use rather more than three molecules . . . in order to complete the action in the same period of time. If the mass of the alkaline carbonate be four times that represented in the equation the action is complete in five minutes." 3, "that increasing the mass of the individual molecules of the precipitant tends to increase the amount of chemical change brought about in a stated period of time," the results indicating that when potassium carbonate is used a larger quantity of calcium carbonate is formed in a given time than when sodium carbonate is employed, except when the action is allowed to proceed during very extended periods. 4, that elevation of temperature tends to increase and on the other hand that dilution of the solutions tends to diminish the rapidity of the change.

These last results are cited in favor of the theory that the action depends upon the motion of the molecules in solution and "that any circumstances which would increase the excursions of the

residual molecules of sodium or potassium carbonate and of calcium chloride, would also increase the chances in favor of collision occurring between these molecules, and hence the chances in favor of the production of larger quantities of insoluble calcium carbonate. On the other hand, any circumstances tending to decrease the excursions of these residual molecules, or tending to increase the number of molecules which take no part in the production of calcium carbonate, and hence to decrease the chances of the sodium or potassium carbonate and the calcium chloride molecules coming into collision, would also tend to diminish the amount of calcium carbonate produced. Raising the temperature at which the two salts are allowed to react upon one another would tend to increase the number of excursions of the different molecules; diluting the solutions would tend to decrease the chances of collision between the two sets of molecules." J. P. C., JR.

13. *Nitrification.*—In the Journal of the Chemical Society just referred to, Mr. Robert Warrington gives the results of a very interesting series of experiments which confirm the theory that nitrification is due to the action of an organized ferment and is probably a function of some low form of vegetable life. This theory was regarded as probable by Pasteur as long ago as 1862, and during the last year has been substantiated by Schloessing and Müntz (Compt. rend., lxxxiv, 301). These experimenters had sought to establish two points first, that antiseptic vapors prevent nitrification; secondly, that nitrification may be induced by seeding with a substance already nitrifying. Mr. Warrington fully confirms the results of Schloessing and Müntz on both these points and adds the important fact that darkness is apparently essential to the action of the nitrifying germs. In a postscript to his paper under date of January 4th, Mr. Warrington refers to a paper of Messrs. A. Downes and T. P. Blunt, read before the Royal Society on December 6th, in which it is shown that light is inimical to the development of bacteria, a few hours exposure to daylight being in many cases sufficient to destroy all the germs existing in an organic fluid, while similar solutions kept in darkness developed bacteria freely. The bearing of this observation on the fact that light greatly retards or even prevents nitrification is obvious. The influence of light on nitrification was not apparently quite unknown; it is twice hinted at in Gmelin's Chemistry (Cavendish Translation, iii, 68, vii, 92) but it is not mentioned by most writers on the subject. J. P. C., JR.

14. *Re-examination of some of the Haloid Compounds of Antimony*, by Professor J. P. Cooke, Jr. (Proceedings of the American Academy of Arts and Sciences, vol. xiii. Boston, 1877).—An extended abstract of Professor Cooke's paper upon the atomic weight of antimony has been given in this volume of the Journal, pp. 41, 107. The present paper contains a description of the chemical and crystallographical relations of the iodide of antimony in particular, but also of the chloride, bromide, and the oxichlorides, oxibromides, and oxi-iodides. The chloride and the bromide of

antimony were both obtained by several different methods, in distinct crystals. These were extremely deliquescent, but, notwithstanding, measurements were made of sufficient accuracy to prove that they both belonged to the orthorhombic system, and that in their axial lengths they were closely related. The iodide was obtained in three crystalline conditions—hexagonal, orthorhombic and monoclinic. The *hexagonal* crystals have a deep ruby-red color; their specific gravity is 4·848, and the melting point is 167°. The *orthorhombic* crystals have a greenish-yellow color; they are formed when the iodide is volatilized at a low temperature—below 114°, and when subjected to a higher temperature they change into the red, hexagonal modification.

In discussing the morphological relations of the hexagonal iodide, and the orthorhombic chloride and bromide, Professor Cooke finds that though belonging to a different crystalline system, the iodide "is constructively closely isomorphous with the two allied compounds." The relation shown to exist is analogous to that which has often been shown to connect an orthorhombic prism of 60° or 120° with a hexagonal form.

The author calls attention to the views previously expressed by him where he had showed "that by a kind of inter-laminar macling the orthorhombic crystals of foliated minerals frequently imitate the structure as well as the forms of the hexagonal system." The theory had also been advanced by him that the true hexagonal forms might be the result of a "molecular macling," according to which theory the crystalline molecules of these forms are groups of three simpler molecules, each of which is a unit in itself, and which probably always becomes isolated when the substance is converted into vapor. It is assumed that the members of these groups are united among themselves in the same relative positions as the diagonals of a regular hexagon, so that the optically uniaxial character of hexagonal crystals is an effect of such grouping, and the hexagonal form an obvious result of the juxtaposition of the six-sided group. Further it is supposed that the simpler molecules are of such a nature that when united as individuals in positions parallel to each they would form crystals having a rhombic section of 60° and 120°.

The discovery of the orthorhombic modification of the iodide, having angles of 60° and 120°, and at the same time optically biaxial, and the fact that it changes by a slightly elevated temperature into the hexagonal variety, is considered by the author as strong confirmation of the theory he has advanced. It being thus shown that the same external form is compatible with the differences which distinguish the orthorhombic and hexagonal system, and the conditions under which one of these isomers changes into the other, indicate clearly that the difference between the two substances is simply a difference in the grouping of the same molecules, and also that in the red modification the molecules are more intimately united than in the yellow.

The monoclinic variety of the iodide is obtained in crystals in

the following manner:—A solution of the iodide in carbon disulphide, after it has been exposed for some hours to the action of the sunlight, undergoes partial oxidation, some iodine being set free, this solution is then distilled over a water bath, and the process continued until almost the whole of the free iodine is removed; the residue is again dissolved in the same solvent, the solution allowed to evaporate spontaneously, there is then obtained a large crop of the red hexagonal crystals, and finally a small quantity of the yellow monoclinic variety. They have a specific gravity of 4·768. That they are true isomers of the other forms of the iodide is shown by chemical analysis, and more strikingly by the ease with which they change into the red hexagonal variety (like the orthorhombic) at a temperature of about 125°.

The form of the monoclinic crystals is in some respects closely related to that of the orthorhombic crystals, for instance in the dimensions of the basal section. Moreover in the re-crystallization of the monoclinic iodide in pure carbon disulphide the hexagonal iodide is obtained, as also a small quantity of minute rhombic plates, whose angles were 60° and 120°. From these facts it is concluded that the monoclinic variety differs chemically from the orthorhombic only in containing a small amount of impurity. The molecules are supposed to be similarly constituted and similarly grouped, but by the adhesion of the impurity to the molecules a certain difference in form results.

The suggestion is made that possibly the supposed orthorhombic crystals may prove upon more exact determination on better material, to be really monoclinic, so that the difference between the two yellow varieties that have been named would exist only in habit. This would not, however, affect the general result reached as to the relation of their form to the hexagonal kind and the reason for its existence.

The "molecular macling" which has been explained is quite distinct from the "interlaminar macling," described by the author in his paper upon the Vermiculites, and alluded to above. The latter involves no essential change in substance; the former produces a new, though isomeric compound. The red and yellow iodides of antimony are as different substances as calcite and arragonite, and the difference in the two cases is conceived to be of the same kind.

E. S. D.

15. *The Telephone, an Instrument of Precision.*—The applications to which the telephone may in future be put cannot yet be all foreseen. I have to-day had its value shown to me in a remarkable way. 1. I used a thermo-electric intermittent current by drawing a hot end of copper wire along a rasp completing the circuit. A telephone was put into the circuit, in another room, and every time that the wire was drawn along the rasp a hoarse croaking was heard in the telephone. 2. I used a thermopile with a Bunsen burner shining on it from a distance of six feet. The current was rendered intermittent by the file, and the sound was most distinctly heard. A Thomson's reflecting galvanometer was introduced into the circuit which showed that the currents

were extremely small. 3. The feeblest attainable currents were now tried. The thermopile was removed, and without any artificial application of heat it was shown by the galvanometer that the natural differences in the temperatures of the different junctions in the circuit were sufficient to generate feeble electric currents only just perceptible with the mirror galvanometer. These were easily detected by aid of the rasp and the telephone. Even when contact was simply made and broken with the hand, a click was heard in the telephone. 4. Lastly, these feeble currents were rendered still more insignificant by passing them through the body of a friend who held one end of the wire in each hand, and still the effects were faintly audible. Here the galvanometer, which was still in circuit, hardly gave any indication.

I have now added the telephone to the list of apparatus in the laboratory, considering it to be perhaps the most delicate test of an electric current which we possess.

In these experiments only one telephone is used, viz: at the receiving end. Employed in this way with a powerful current sent from the other end of the line, we may hope to have messages sent through submarine cables much more rapidly than at present. Probably it will be best to have the intermittent nature of the current maintained by an induction-coil, or by a spring rubbing against a continuously rotating cog-wheel, when the current is allowed to pass only when required by the depression of a key which communicates to the listener at the receiving end the long and short dashes of the Morse alphabet.

I ought to mention that I believe the person who first used a thermo-electric current with a telephone was Prof. Tait.

GEORGE FORBES, Andersonian College, Glasgow, February 13. —*From Nature of Feb.* 28.

II. GEOLOGY AND MINERALOGY.

1. *Origin of the Driftless Region of the Northwest;* by ROLAND D. IRVING.—In a notice of vol. ii of the Geology of Wisconsin, given in this Journal for January last, the explanation I have offered for the existence of the driftless region of Wisconsin and the adjoining States is said to be, "though different," closely related to that given by Professor Winchell in his Minnesota Geological Report for 1876. I am informed, also, that a review of the Wisconsin Report, written for a Chicago journal —but which I have not myself seen –has said that my explanation is the same as his. From these statements it might be inferred that I had merely followed in his track. It is entirely true that his views have some analogy to mine, and that they were first published; but my report had all been, not only written, but stereotyped and *printed* before the Minnesota volume came to hand, in the fall of 1877; so that, whatever my conclusions are worth, they are wholly my own. Possibly they are worth the more because analogous to those reached independently by some

one else. The matter has long excited my attention, the entire inadequacy of the older views having forced itself upon me when first engaged in field-work in Wisconsin.

However, though cheerfully acknowledging the priority of Professor Winchell's views so far as they cover the same ground with mine, I find, on examining his report again more closely that not only do we reach our conclusions in quite different ways, but that the analogy between his theory and mine is, after all, not much more than slight; and, moreover, that in some of the main points of his I cannot acquiesce at all. His idea is, as I understand it, that the large area of Archæan rocks lying south of Lake Superior exerted a protecting influence upon the region to the southward, crowding back the ice, which had been split by the high ridge of Keweenaw Point, one part flowing westward on the north side of the point, and so passing by the driftless region on the north, the other turning southward, and so to the *east* of the driftless area. Now I assign *nothing* to the protecting influence of the crystalline rock region, but *everything* to the deflecting influence of the already existing depressions of Lake Michigan, the Green Bay Valley and Lake Superior. The crystalline rock region is no such lofty one as he appears to suppose, is almost everywhere heavily drift-covered, and over most of its surface shows sign of the strongest glaciation. The deep trough of Lake Superior, over 2,000 feet in depth in its western portions, allowed the passage from it of only the uppermost portions of the ice. These, divided into separate tongues by the transverse ridge topography of the region south of the Lake, and by it deflected toward the W.S.W., overtopped the water-shed, and carried morainic drift far down on the southern slope; but, because of their diminished size and force, and of their westerly direction, they left the region farther south untouched.

Professor Winchell's view was reached by noticing the relation in position of the driftless region and the area of crystalline rocks, which relation, in my view, is an accidental one, except so far as the position of the trough of Lake Superior, carved in the soft sandstones of the Lower Silurian and Keweenian, is affected by the position of the Archæan area. My own conclusions were reached by the absolute demonstration of the former existence of separate glaciers in the troughs of Green Bay and Lake Michigan, which is afforded, as I think, by the facts given by Professor Chamberlin and myself in our reports on Eastern and Central Wisconsin. At the time that I wrote, the proofs of a similar state of affairs for the Lake Superior country were nearly as good, and are now complete, as will be seen when the reports on that country come to be published.

University of Wisconsin, February 18, 1878.

2. *Second Geological Survey of Pennsylvania.*

(1) *Report of Progress in the Fayette and Westmoreland district of the Bituminous Coal Fields of Western Pennsylvania;* by J. J. STEVENSON. Part 1, Eastern Allegheny County and Fayette

and Westmoreland Counties, west from Chestnut Ridge. 438 pp. 8vo, with maps and sections.—Professor Stevenson's Report gives first an account of the physical features and general geology of the district, and then describes with detail the stratification of the rocks in its different parts, the relations of the several beds to those of the adjoining regions, the distribution of the included coal and iron-ore beds, and various details as to the special characteristics and variations of the beds. The region lies just to the east of Pittsburgh and hence includes the great Pittsburgh coal bed. The coals and iron ores are described, and many analyses are given of the latter.

(2.) *Report of Progress in the Cambria and Somerset District of the Bituminous Coal Fields of Western Pennsylvania;* by F. and W. G. PLATT. Part II, Somerset. 348 pp. 8vo. Harrisburg, Pa.—The region here described lies to the southeast of that treated of in Prof. Stevenson's report. It contains to a large extent the same rocks and coal beds, but with many peculiarities in their distribution that give special interest to the facts brought out. Numerous sections are given which are carefully described, and the subject of the variability of the coal beds is treated of. The volume contains a preface by Professor J. P. Lesley, Director of the Geological survey of the State, announcing that the "Elk Lick Coal," stated in the final Geological Report of 1858 to occur at the bottom of the Barren Measures, and at the top of the Lower Productive Coal series, has no existence, and that it should be expunged from that report wherever it occurs, and also from Lesley's "Manual of Coal." The Report is illustrated by numerous wood-cuts, and six maps and sections.

(3.) *Oil Well Records and Levels;* by JOHN F. CARLL. 348 pp. 8vo. Harrisburg, Pa.—This Report is a very valuable systematised statement of facts connected with the oil deposits of Western Pennsylvania. It contains the geological and geographical positions and depths of all the oil openings, and sections of the rocks in each case as far as they were obtainable. The work gives records, "more or less doubtful," of 2,000 wells; and Prof. Lesley observes, in a prefatory note, that the number might have been 20,000, if the records had been in all cases preserved. This volume is to be followed by another by Mr. Carll in which all the facts here given will be combined and explained in a manner better adapted to the popular reader; and in it the geology of the rocks penetrated by the oil borings, and of the oil level, will be specially considered, with other matters of scientific and practical value.

3. *The Ancient Life-history of the Earth*, by Professor H. ALLEYNE NICHOLSON, Professor of Natural History in the University of St. Andrews, Scotland. 408 pp. 8vo. (New York: Appleton & Co.).—The work is, as the title page states, a comprehensive outline of the principles and leading facts of Paleontological science. After chapters treating in a general way of the fossiliferous rocks, their succession and the uses and relations of the fossils

they contain, it takes up in succession the life of the periods in geological history, commencing with the oldest. Its many illustrations are not as well engraved as they should be. The author is a zoologist as well as geologist, and the student will find his work a very valuable help toward obtaining a comprehensive knowledge of the progress of life on the globe.

4. *Reports of the United States Exploration of the 40th parallel*, CLARENCE KING, Geologist in charge. Submitted to the Chief of Engineers and published by order of the Secretary of War, under authority of Congress. Two volumes of these Reports, on the region in the vicinity of the 40th parallel between the Sierra Nevada and the Front Range of the Rocky Mountains, have recently been issued. They add greatly to our knowledge of Rocky Mountain geology, and are hastening on the time when we shall have a completed map of the great territories. These two volumes are numbered volumes ii and iv. Volume iii, by Mr. King, is now in the press, and will soon be ready for delivery.

Volume ii, contains the "*Descriptive Geology*" by ARNOLD HAGUE and S. F. EMMONS; it is an octavo volume of 890 pages and is illustrated by twenty-six plates. A notice of it is deferred to another number of this Journal.

Volume iv, which extends to 670 pages, consists of three reports or parts: I, *Paleontology*, by F. B. MEEK, illustrated by seventeen plates; II, *Paleontology*, by JAMES HALL and R. P. WHITFIELD, illustrated by seven plates, and III, *Ornithology* by ROBERT RIDGWAY. The Report of the late Mr. Meek contains fossils ranging from the Lower Silurian to the Tertiary. The oldest species are two Trilobites from Antelope Springs, House Range, Utah, named *Conocoryphe* (*Ptychoparia*) *Kingii*) Meek, and *Paradoxides? Nevadensis* Meek.

Professor Hall and Mr. Whitfield describe Potsdam fossils from the Eureka District, Nevada; and the west side of Pogonip Mountain, White Pine District, Nevada. They include Brachiopods, of the genera *Obolella*, *Lingulepis*, *Kutorgina* and *Leptæna*, also Trilobites of the genera *Crepicephalus* (7 species), *Conocephalites* (*Pterocephalus*) (1 sp.), *Ptychaspis* (1 sp.), *Chariocephalus* (1 sp.), *Dicellocephalus* (3 sp.), *Agnostus* (4 sp.) The other fossils belong to the upper portion of the Lower Silurian, the Devonian, Carboniferous, Triassic and Jurassic formations.

Mr. Ridgway's report contains much on the habits of the various birds observed by him while in the field, and also on their distribution and variations. A general review of the several local regions and their faunas is presented, and lists of the species of each, which we propose to notice in a future number.

5. *Report on the Clay Deposits of Woodbridge, South Amboy, and other places in New Jersey, together with their uses for fire brick, pottery, etc.*, by GEORGE H. COOK, State Geologist of New Jersey. 382 pp. 8vo. Trenton, N. J., 1878.—This valuable volume, on the New Jersey clay deposits, is published by the State as a part of the results of the Geological Survey. It gives,

in the first place, the general geological and geographical facts connected with the clays, and afterwards describes the various openings, and the character and composition of the clays they afford. It closes with a chapter on the economical uses of the clays and on the methods of digging. It has two maps. One shows the distribution of the clays across the State of New Jersey, along a band, five to eight miles wide, from South Amboy, southwest to Trenton, on the Delaware. The second is a large colored map, giving special details with regard to the northeastern portion of this tract, and covering a surface of about ten miles square, with contour lines for every ten feet.

6. *The First and Second Glacial eras of Europe.*—The third number for 1878 of Petermann's "Geographischen Mittheilungen" contains a colored chart, showing the condition of Europe in the first and second Glacial eras, according to the views of H. Habenicht, prepared, the author states, from the best sources. It represents, by colors, the distribution of the glaciers of the two eras, and their difference in length. Mr. Habenicht holds, contrary to the opinions of many geologists, that the boulders were distributed by icebergs; and consequently he places Northern Europe, down to nearly latitude 50°, under the sea, and contracts the glaciers of Scandinavia to the valleys on the western side of the range. The chart is interesting as exhibiting the supposed condition of Europe, according to one believing in the iceberg theory of the drift.

7. *Cordaites with flowers from the Coal region of Pennsylvania.*—Mr. Lesley communicated part of a letter from Mr. L. Lesquereux, of Columbus, Palæobotanist of the Geological Survey of Pennsylvania, relating the recent discovery by Mr. Mansfield, in his coal mines near Darlington, Beaver Co., Pa., of the flowers and leaves attached to the stems of *Cordaites*. "Sternberg in 1835, first found a stem with leaves attached; on which specimen, Corda, in 1845, made his celebrated analysis of *Flabelluria borassifolia*. One other specimen was found by Lesquereux near Potsville in 1858. Recently, Grand 'Eury's discoveries enabled him to publish last fall his splendid monograph of the *Cordaites* in his Carboniferous Flora. Mr. Mansfield has now obtained a splendid series of branches with leaves, and even with leaves and flowers, representing in well defined characters several species, and a new section of this family unknown to Grand 'Eury; so that we now have not only the types of the French author, but other and some new ones far more clearly illustrating the relation of this remarkable group."—*Amer. Phil. Soc.*, Feb. 1, 1878.

8. *On the Mineral Caves of Huallanca, Peru;* by HENRY SEWELL, F.R.G.S.—Mr. Sewell has given in the London Mining Journal an account of his visit, made in 1875, to the mineral caves of Huallanca. The silver-producing caves are situated upon the eastern flank of the Peruvian Andes at an altitude of 14,700 feet above the sea, and 4000 feet above the town of Huallanca. The region was reached after an arduous journey across the Andes

from the port of Casma; in the course of it, it was necessary to cross several parallel ranges, one of them 16,800 feet in altitude, and another 17,200 feet. The silver mines lie in the heart of a coal formation which has been upheaved so that the strata now stand nearly vertical. Numerous seams of coal occur near the mines. The mass of the argentiferous ores consists of the mineral tetrahedrite, and they contain about 800 ozs. to the ton. The ore is obtained in the shaly portion of the formation, as well as in the sandstone. In the latter it is found in huge "vughs" or caves, many of these being as much as twenty-five to thirty feet long, and of about the same depth. These caves are coated with from two to three inches of argentiferous ores and millions of crystals of tetrahedrite are destroyed by the picks of the miners who break down the ore in that manner. [The specimens of tetrahedrite brought back by Mr. Sewell are very remarkable for the unusual size and brilliancy of the crystals; some of these he has presented to the Yale Museum].

9. *On the new Mineral Homilite.*—In December, 1876, M. Paijkull published an account of a new mineral, associated with meliphanite and erdmannite on the island of Stockoë, near Brevig, Norway, to which he gave the name of *homilite*. The mineral was of a black or brownish-black color, and in composition was shown to be related to datolite, it containing a considerable amount of boracic acid. The same mineral has been investigated by MM. Des Cloizeaux and Damour (Ann. Chem. et Phys., V, xii, 1877). M. Des Cloizeaux has shown that in crystalline form it is closely isomorphous with gadolinite and datolite. Moreover the optical examination has shown it to be still more nearly related to the former mineral in that some crystals are entirely doubly refracting, others have a yellow crust of singly-refracting mineral, and others are totally isotropic. M. Damour has analyzed the mineral and his analysis (2) is here quoted together with that of M. Paijkull (1).

	SiO_2	B_2O_3	Al_2O_3	Fe_2O_3	FeO	MnO	Ce_2O_3*	MgO	CaO	Na_2O	K_2O	H_2O	
(1)	31·87	[18·08]	1·50	2·15	16·25	--	--	0·52	27·28	1·09	0·41	0·85	=100·
(2)	33·00	[15·21]	--	--	18·18	0·74	2·56	--	27·00	1·01	--	2·30	=100·

* With La_2O_3, Di_2O_3.

The hardness is 4·5, and the specific gravity 3·34 (Damour). From the fact that the analyses do not afford any simply atomic relations, and from the results of his optical examination, M. Des Cloizeaux concludes that the singly refracting portions of the crystals have been so changed as to form with doubly refracting parts a mixture of varying proportions and hence of variable composition.

III. Botany and Zoology.

1. *Flora of Tropical Africa;* by Daniel Oliver. Vol. III. *Umbelliferæ* to *Ebenaceæ*.—In the present continuation of this work Professor Oliver has secured an efficient collaborator in Mr. Hiern, who has not only taken the *Ebenaceæ*, of which he has formerly published a classical monograph, but also the *Umbel-*

liferæ, *Rubiaceæ* and *Dipsaceæ*, and has borne a part in the elaboration of the *Compositæ*. The latter order is represented by more genera than would be expected, namely 117, and 17 are peculiar to this Flora, all small or monotypic. The only large genus is *Vernonia*, with 78 species. The *Rubiaceæ*, which occupy a slightly larger number of pages (215) have 78 genera, 30 of which are endemic, and three are new. We observe that *Tarenna* Gærtn., replaces *Webera* of Schreber, being three years older; also *Chomelia* Linn., is said to be the correct name, but *Chomelia* of Jacquin is kept up. *Canthium* Lam., is restored in good time for that important genus, much to the relief of the nomenclature, and the original *Plectronia* of Linnæus is said to be *Olinia!* The Liberian coffee, the seed of *Coffæa Liberica*, "is said to be far superior to *C. Arabica*, having larger berries and a finer flavor, and being at the same time more robust and productive." *Psychotria*, with 61 species, is made to include *Chasalia*, and to exclude *Grumilea*, which is restricted to Old World species. *Richardia* Linn., is taken up in place of *Richardsonia* Kunth, to the detriment of the common *Aroideous* genus which has so long borne this name. It is well known and duly recorded that the Rubiaceous genus was named in honor of Mr. Richardson; wherefore it should have been written *Richardsonia*. There was not absolute need to change the form; but this having long ago been done, and another plant named *Richardia*, in honor of L. C. Richard, and the present state of things having been acquiesced in for more than half a century, this certainly is a case to which the rule "*quieta non movere*," and Lord Melbourne's deprecation, "Can't you let it alone?" would well apply. A. G.

2. *Ferns of North America;* by Prof. D. C. Eaton. Part III has appeared with remarkable promptitude, and has more than usual interest. There is a characteristic plate of *Aspidium Noveboracense;* another devoted to *Camptosorus rhizophyllus*, and to *Aspidium pinnatifidum* (to the latter of which, rather than to the former, *A. ebenoides* may have some blood-relationship); and the third is given to *Notholœna Fendleri* and *N. dealbata*, both species well adapted for color-printing by their whitened fronds, delicate subdivision, and brown stalks. A. G.

3. *Report on the Fossil Plants of the Auriferous Gravel Deposits of the Sierra Nevada;* by Leo Lesquereux. With 10 plates. 4to. 1878. (Vol. vi, No. 2 of the Memoirs of the Museum of Comparative Zoology at Harvard College.)—A memoir beautiful in its dress and very interesting in character. The specimens figured are impressions of leaves, "imbedded in a fine-grained whitish clay or soapstone," mainly from a collection made by Mr. Voy, of Oakland, California, which was secured and presented to the California University by the liberality of Mr. D. O. Mills. These Pliocene remains are of special interest from the light they throw upon the archeology of the existing North American flora; and we shall probably find occasion and opportunity to discuss them in this relation. A. G.

4. Elias Magnus Fries of Upsal, and one of the most distinguished and revered of botanists, born on the 15th of August, 1794, died February 8, 1878. He had worthily occupied the chair of Linnæus at the University of Upsal for almost half a century; and his death, a month after the celebration of the 100th anniversary of that of Linnæus, marks the close of an era. A. G.

5. *Catalogue of the Collections in the Museum of the Pharmaceutical Society of Great Britain*, compiled by E. M. Holmes, F.L.S., Curator of the Museum. 1878. pp. 302.—About two-thirds of this catalogue relate to the vegetable materia medica; the objects all classed under the names of the plants producing them with interesting descriptive and critical remarks. A. G.

6. *Thuret's Garden at Antibes*, as already announced, has been turned over to the French Government, in order that its usefulness may be continued. It is now decided that it is to be a sort of Mediterranean branch of the Jardin des Plantes of Paris. Botanists will be glad to know that M. Naudin, the best of horticultural botanists, is to have the charge of it. The climate of Antibes is especially proper for Utah, Arizonian and South Californian plants; and we trust our collectors and amateurs will keep this in mind, and that many seeds, bulbs, succulent plants, etc., may be supplied to this important establishment, where they may be kept in cultivation. The Botanic Garden of Harvard University will receive and forward any American collections destined for it. A. G.

7. *Circumspection of Ants.*—Professor Leidy states that, a number of years ago, he had been led to an observation on the little red ant, sometimes a great pest to our dwellings, which would indicate a ready disposition to become circumspect. When he purchased his present residence, while it was undergoing some repairs, he noticed a fragment of bread, left by a workman in one of the second-story rooms, swarming with ants. Apprehending that the house was seriously infested, to ascertain whether it was so he placed a piece of sweet cake in every room from the cellar to the attic. At noon every piece was found covered with ants. Having provided a cup of turpentine oil, each piece was picked up with forceps and the ants tapped into the oil. The cake was replaced, and in the evening was again found covered with ants. The same process was gone through the following two days, morning, noon, and night. The third day the number of ants had greatly diminished, and on the fourth there were none. He at first supposed the ants had all been destroyed, but in the attic he observed a few feasting on some dead house flies, which led him to suspect that the remaining ants had become suspicious of the sweet cake. He accordingly distributed through the house pieces of bacon, which were afterwards found swarming with ants. This was repeated with the same result for several days, when, in like manner with the cake, the ants finally ceased to visit the bacon. Pieces of cheese were next tried, with the same results; but with an undoubted thinning in the multitude of ants. When the

cheese proved no longer attractive, recollecting the feast on dead flies in the attic, dead grasshoppers were supplied from the garden. These, again, proved too much for the ants; and, after a few days' trial, neither grasshoppers nor anything else attracted them; they appear to have been thoroughly exterminated, nor has the house since been infested with them.—*Proc. Acad. Nat. Sci. Philadelphia*, 1877, p. 320.

8. *Carboniferous Articulates of Illinois.*—At the meeting of the Boston Society of Natural History for December 5, 1877, Mr. S. H. Scudder showed some drawings of interesting Articulates from the Carboniferous rocks of Illinois. The first represented a species of white ant, showing a wing without reticulation; the second, the terminal segments of a crustacean belonging to a genus allied to *Dithyrocaris*, but which he had at first taken for some extraordinary form of insect.

9. *A Manual of the Anatomy of Invertebrated Animals*, by Thomas H. Huxley, F.R.S. 596 pp. 8vo. (New York: Appleton & Co.)—The best and latest students' manual of the invertebrates in the English language.

10. *Nomenclature in Zoology and Botany*, by W. H. Dall, U. S. Coast Survey.—Mr. Dall, as chairman of the committee of the American Association on Zoological Nomenclature, has been doing good service to natural science by his efforts to obtain the opinions of naturalists on various points connected with the subject of nomenclature. This pamphlet is the result of further labor in the same direction. It contains, besides a brief report, an Appendix consisting of a resumé of all the principles and rules of nomenclature as heretofore set forth by the chief authorities on the subject, with initials giving the authority for the diverse views, in preparing which the author has evidently expended a great amount of labor and care. He has endeavored, moreover, to combine the principles of zoological and botanical nomenclature into one symmetrical system. Mr. Dall's statement of this subject is a great step toward realizing the uniformity so much desired.

11. *A Pigeon living after the removal of nearly all the Brain.*—Dr. McQuillen described a case of the extirpation of nearly all of the cerebrum of a pigeon by himself, and desired to place on record the fact that the subject not only survived the operation twenty-four days, but gradually regained its usual powers and habits of flight, and its ability to feed itself and drink. Only one such case is on record. He argued for the propriety and usefulness of such operations from the acknowledged existing uncertainties of the science.—*Proc. Amer. Phil. Soc.*, Feb. 1, 1878.

V. Miscellaneous Scientific Intelligence.

1. *The Earthquake of November* 4, 1877.—(Read at the November meeting of the Natural History Society, by Principal Dawson, LL.D., F.R.S.)—In the Canadian Naturalist, vol. v, first series will be found notes on the earthquake of October 17,

1860, with a summary of facts relating to the previous shocks recorded in Canada, and some general remarks on their periods, local peculiarities and probable causes. The subject was continued in vol. i, of the new series, in connection with the earthquake of April, 1864, and in vol. v, new series, in connection with that of October 20, 1870. I may refer to these notices for what is known on Canadian earthquakes up to that time, and we may now continue the narrative in connection with the somewhat wide-spread disturbances of the earth's crust in the present autumn.

On January 4, 1871, a shock was experienced at Hawkesbury, Ontario, but was not reported from any other place. A more extensive earthquake occurred on May 22, 1871. It prevailed from the city of Quebec to the western part of Ontario. The time for Quebec is stated at ten minutes before two A. M., and there was a second shock at twenty minutes past three. The time for Perth, Ontario, is stated at half-past one. It is noteworthy that this earthquake occurred at nearly the same time with that recently experienced. Since 1871 several minor shocks have been noticed from time to time, but did not attract much attention, and I have preserved no details in relation to them.

That of the present month was probably the most considerable since 1871. It occurred at Montreal, at ten minutes before two on the morning of Sunday, November 4. At Montreal there was only one distinct shock, preceded by the usual rumbling noise, and sufficiently severe to be distinctly felt, and to shake window-sashes and other loose objects, causing them to vibrate for several seconds. In so far as the published reports give information, the shock would seem to have been limited to the area along the river St. Lawrence, extending from near Three Rivers on the east, to Kingston on the west, and in a direction transverse to the St. Lawrence from Ottawa to the southern part of New England. In a paper prepared for the American Journal of Science, by Professor Rockwood, of Princeton, he defines the area in question as that of "an irregular trapezium whose angles are marked by Pembroke, Ont., Three Rivers, P. Q., Hartford, Conn., and Auburn, N. Y., and which is some 200 miles on its northern and southern side, about 300 miles on the eastern side, and 175 on the western." So far as can be learned from the reports, the shock seems to have been most severely felt on the north side of the valley of the St. Lawrence and about Lake Champlain, or may be said to have had its center in the Adirondack and Green Mountain region.

In the notice of Canadian earthquakes in 1860, I mentioned that it had been observed that the greatest and most frequent shocks have occurred a little after the middle and toward the close of each century. We are now approaching the latter period, so that possibly the last shock may be the beginning of a series of similar phenomena. Since, however, there is no known reason for this periodicity, it may be a merely accidental coincidence, or may depend on some cycle of about half a century.

If we add to the table of earthquakes in Eastern America, given in vol. v of the Naturalist, the more recent earthquakes observed in Canada, the proportion for the several months will stand as follows:—

January, 9 earthquakes; February, 4; March, 5; April, 5; May, 7; June, 3; July, 4; August, 6; September, 4; October, 8; November, 15; December, 8. Total, 78.

Thus of seventy-eight recorded Canadian and New England earthquakes, fifteen, or nearly one-fifth, occurred in November; forty, or more than half of the total number, in the third of the year, extending from October to January inclusive. The published catalogues show that similar ratios have been observed elsewhere, at least in the Northern hemisphere.

In some earthquakes a low state of the barometer has been observed, as if a diminution of atmospheric pressure was connected with the movements of the crust producing seismic vibrations. This we can readily understand if a low state of the barometer should prevail over an area of the crust tending to rise, simultaneously with a high pressure over a sinking area. In this case a state of previous tension might terminate in a rent of the crust causing vibration. In the present case no very decided indication of such a cause appears, at least in so far as this part of the St. Lawrence Valley is concerned. Mr. McLeod informs me that the mean barometer for the week preceding the earthquake was 29·7564, and for the following week 30·0864. The barometer on the Friday before the earthquake at 8 P. M. was 29·115, the lowest observed since March last; but at 1·50 A. M. on Saturday it was about 29·967, which is very near the mean of November, 1876, and also a little above the mean barometer of the place for the whole year; and on Sunday afternoon it rose to 30·200. It would thus appear that the earthquake was preceded by a low state of the barometer, and followed by one unusually high for the season, and this rapid fluctuation was accompanied with much atmospheric disturbance in the region of the lakes and the St. Lawrence Valley. The weather map issued by the War Department at Washington for Sunday morning, November 4, shows a low barometer in the Gulf of St. Lawrence and a high barometer in the Middle States—the area of the earthquake being about half way between the extremes.

In connection with previous earthquakes it has been observed that the greatest intensity of the shocks appeared near the junction of the Laurentian with the Silurian formations. This would be a natural consequence either of the propagation of vibrations upward from deep underlying regions through the Laurentian rocks, or from the overlying sedimentary rocks towards these older rocks. In the case of the recent earthquake, this appears to have applied chiefly to the border of the Laurentians extending round by the Ottawa and Kingston to the Adirondacks, as if a wave propagated through the Silurian formations had broken against the southern and eastern sides of the Laurentian region,

or a shock originating under the Laurentian of these regions had extended itself from them into the Silurian rocks to the south and east. If the prevailing impression stated in the reports, that the vibrations passed from W. to E. or N.W. to S.E., is correct, the latter would be the more probable supposition. It is, however, very difficult to attain to any certainty as to the actual direction of the disturbance, and some observers give it as precisely the opposite of that above stated.

On the 14th of November a slight shock was felt at Cornwall, Ontario, and on the 15th of November earthquake shocks occurred over a wide area in Kansas, Iowa, Dakota and Nebraska.

OBITUARY.

CHARLES FREDERICK HARTT.—Professor Hartt, according to a telegram from Rio Janeiro, died of yellow fever soon after the middle of March (18th?). Professor Hartt was born about 1838 at St. John, N. B. In his youth he evinced a taste for geology, and discovered at St. John many fossil plants, and the oldest specimens of fossilized insects then known. He studied from 1862 to 1865 under Agassiz at Cambridge, and accompanied that eminent scientist to Brazil as geologist of his expedition. On his return he was appointed Professor of Geology and Physical Geography at Cornell University. After his first visit to Brazil, Professor Hartt acquired a thorough knowledge of the Portuguese language, and that empire became his favorite field of study. He returned three times and zealously explored the northern provinces, giving most attention to the valley of the Amazon. During one of these expeditions, undertaken under the auspices of the Hon. Edwin D. Morgan of New York, he sent home an interesting series of letters for publication. Papers containing some of the geological results of these expeditions are noticed in this Journal in volumes i, iv, vii, viii and x, of the third series (1871 to 1875). In May of 1875 the Brazilian Government placed Professor Hartt in charge of the Geological Survey of the Empire, and gave him a liberal salary; since then he has been carrying forward this great work. The results thus far obtained have been only partially published. A translation of his first Report of Progress, made by Professor T. B. Comstock, is contained in this Journal in vol. xi, 1876 (p. 466), and a brief announcement of further discoveries, in vol. xii (p. 464). In 1870, he published his principal work, "The Geology and Physical Geography of Brazil." He felt at home in Rio, where he enjoyed excellent health, and was greatly esteemed. His family reside at Buffalo, N. Y. His death is a great loss to the scientific world.

ANGELO SECCHI, the Astronomer and Director of the Observatory at the Collegio Romano at Rome, Italy, died on the 26th of February last. Father Secchi, in the years 1848 and 1849, was connected with the Observatory of Georgetown College, near Washington. In 1850 he returned to Europe and entered on his labors at Rome. His papers on astronomical and physical subjects are very numerous and of great value.